SCIENCE.

FRIDAY, OCTOBER 12, 1883.

HERMANN MÜLLER.[1]

THE sad news has just reached this country of the death of Professor Müller, at Prad, on the 25th of August.

Since the death of Mr. Darwin, Dr. Müller has occupied the position of most prominence among students of the mutual relations between flowers and insects, — a study which, in the last decade, has contributed as much as any branch of biology to the substantiation of the main points of adaptive evolution. Müller was born at Mühlberg, Sept. 23, 1829, and was a younger brother of the well-known Brazilian naturalist, Fritz Müller, much of whose work has passed through his hands before its publication.

Between 1848 and 1852 he studied at the universities of Halle and Berlin, devoting himself to natural history. In the latter year he passed the Oberlehrer examinations, and served his novitiate in the Berlin realschule. In 1854 he received his first appointment as teacher in the school at Schwerin, and the following year took the natural sciences in the realschule at Lippstadt, where he remained as teacher and director until his death.

Previously to the attainment of his degree, Dr. Müller had shown considerable zeal in natural history explorations, which were continued, in 1855, in the vicinity of Krain, where he did some especially interesting work on the blind insects found in the caves at this place, the results of his studies appearing in the *Stettiner entomologische zeitschrift* for 1856–57. After settling at Lippstadt, he gave particular attention to botany and entomology, working up, in particular, the local phenogamic flora, and later the mosses of Westphalia, sets of which were distributed by him between 1864 and 1866.

About this time the classical work of Darwin on the fertilization of orchids by insects directed his attention to the pollination of flowers, — a subject, which, neglected since the time of Sprengel, was then attracting several biologists. His familiarity with Westphalian plants and insects fitted him especially for work of this nature; and his first contributions[2] showed that he was also possessed of the requisite powers of observation and interpretation.

From this time on, his leisure was given to field-work in this specialty, many of his summers being spent in the Alps. While Delpino, Hildebrand, and others were not slow to follow in the steps of Mr. Darwin, showing, both from the structure of flowers and the results of many careful experiments, how they must *a priori* be fertilized, Müller observed, in addition, how their pollination is actually effected; and our knowledge of the degree to which the reciprocal adaptations of flowers and their visitors extends may be set down as in large part the result of his labors.

In the past ten years, numerous papers from

[1] The portrait on this page is engraved from a photograph by Ophoven of Lippstadt, kindly furnished by Prof. William Trelease of the University of Wisconsin.

[2] Beobachtungen an Westfälischen orchideen (*Verhandl. naturh. ver. Preuss. Rheinl. u. Westfälens*, 1868) and Anwendung der Darwinsche lehre auf bienen (*ibid.*, 1872).

his pen have appeared in the *Botanische zeitung*, *Bienen zeitung*, *Kosmos*, *Nature*, etc., while, as editor of the department of Justs' *Jahresbericht*, relating to pollination and dissemination, he has contributed reviews of all of the more important publications bearing on his specialty. Beside these, he published two books, — Befruchtung der blumen durch insekten, und die gegenseitigen anpassungen beider (which appeared in 1873, served as the basis of a very instructive series of articles in *Nature*, and was largely drawn upon by Lubbock in the preparation of his little work on British wild-flowers, and which, supplemented by the more recent observations of its author, has lately been translated into English) ; and Alpenblumen, ihre befruchtung durch insekten und ihre anpassungen an dieselben (a volume of equal size, published in 1881, and, like its predecessor, filled with instructive facts).

From the first, Dr. Müller was a pronounced evolutionist, perhaps erring in too exclusive contemplation of a limited part of the evidence of derivation, and, like many others of the German school, inclined to push evolutionary logic to its ultimate if undemonstrable conclusion of materialism.

As a teacher he was most excellent, having the faculty, not only of imparting ideas to his pupils, but of inspiring their enthusiasm. In his specialty he was a careful observer, noting and accounting for many minute structural peculiarities in both flowers and insects, which, so long as their utility remained undiscovered, were explicable only by the theory of types in nature. So far as observation is concerned, his work is above criticism. As a rule, too, his inferences are correctly drawn, though the limitation of his studies to a small part of the world has at times rendered his enthusiasm over the biological significance of some supposed new adaptation, subject to the criticism of specialists previously familiar with the structure, if not with its meaning.

As a friend, Dr. Müller was always cordial, ever ready with encouragement and assistance for younger workers in the line of his specialty. He had, however, little patience with inaccuracy in observation, and, both publicly and in private, criticised errors with vigor; but, though his criticisms were sometimes severe, they were seldom unkind, and never unjust. By his death, biological science loses not only one of its most enthusiastic and able devotees, but also one, who, by the independent and thorough nature of his work, may be styled not inappropriately an epoch-maker.

THE USE OF THE SPECTROSCOPE IN METEOROLOGY.

In April last it was thought desirable to add to the regular meteorological observation made at the Shattuck observatory, Dartmouth college, the hygrometric indications of the spectroscope. The observations were made in accordance with the directions of J. Rand Capron in his 'Plea for the rain-band.' The instruments used were two direct vision spectroscopes: one a 3½-inch 'vest-pocket' instrument of Hofmann's; the other 10 inches in length, and capable of separating the *D* lines with direct sunlight. The observations made in this way were found to be interesting, but unsatisfactory. The difficulty which an observer must always find in estimating confidently the degree of intensity of the absorption lines and bands with the widely varying lights of fair and cloudy weather, makes the arrangement of some method of measurement very desirable. After a few trials in other directions, the device described below was decided upon, and has proved satisfactory. It was thought that the absorption *lines* of aqueous vapor, seen with a spectroscope of rather high power, are better adapted to delicate measurement than the broad *band* seen with a low power. The small spectroscope used shows the dark band on the red side of the *D* line with great clearness; but the absorption lines are only visible when particularly strong. With the larger instrument, however, the spectrum is so elongated that the general darkening near *D* is hardly noticeable; while the two moisture lines to be found there are very prominent. The apparatus illustrated is designed to measure the variation in intensity of the darker line of this pair (the *a* of the *D* group of Janssen's map).

The only methods of measurement of the intensity of absorption lines, known to the writer, are those of Janssen and Gouy. The

former, in 1871, in his work in mapping the atmosphere lines, used for comparison black lines of various widths, ruled on white paper, and viewed through vessels filled with darkened water.[1] Gouy made some measurements of solar lines by photometric methods; isolating a narrow strip of the spectrum adjacent to the line, and comparing its light with that of a strip of equal width containing the line. From these data he calculated the intensity of the line, not in photometric, but in linear units.[2] The method adopted by the writer is entirely different from either of these; and, as far as known, is new.

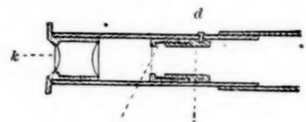


FIG. 1.

What was desired was the production of an artificial absorption spectrum, the intensity of whose lines could be varied at will, until one of the lines thus produced should be sensibly the same as the line to be measured. Fig. 1 is a section of the attachment to the spectroscope made for this purpose. The dark lines required are diffraction fringes produced at the focus of the positive eye-piece, which are therefore seen projected on the spectrum. They are produced by placing a silk fibre a little beyond the focus of the eye-piece. In the figure, the piece *a* slides in the tube, bearing with it a single silk fibre placed vertically and just in the middle of the field of view of the eye-piece. The fibre is maintained vertical by means of a projecting pin sliding in a longitudinal slot in the tube, as shown at *s* in fig. 2. The sliding motion is given to it by means of the piece *b*, which turns freely, but cannot slide, being retained by the screw *d* fitting in a groove made entirely around the piece. Two openings are made in the tube, on opposite sides, so that *b* can be turned directly with the fingers. One of these windows is shown at *n* in fig. 2. By turning in one direction, the silk fibre may be put nearly in the focus: by turning back, it can be made invisible. When near the focus, the fibre appears as a pair of dark parallel lines and quite close together. As it is drawn away from the focus, the lines appear to separate somewhat, growing constantly fainter until they disappear. The fainter diffraction fringes produced are invisible in the rather weak light of the spectrum. Whole revolutions of the screw *b* are read off on the graduation at the side of the slot, and fractions (tenths) are read from the piece itself, which is graduated as a micrometer screw. The lines thus produced resemble closely the *D* group, particularly when both are strong, when a very sharp eye is required to distinguish the spurious lines from the genuine. As the movement of the eye from side to side would modify the appearance of the interference lines, making one darker than the other, the spectrum must be viewed through a narrow vertical opening, making such motion impossible. For this purpose a piece of black paper (not shown in the figure), provided with a vertical slit of perhaps 0.7 mm. width, must be placed on the eye-lens at *k*. Even with this, a little care is necessary in the position of the eye, that the pair of lines shall always be equal. The slight darkening of the spectrum between the two lines, which occurs, is in this case not objectionable, as it imitates pretty closely the general absorption in the space between *D* and the *a* line of the *D* group. The instrument, as figured, is provided with a tangent screw at *e*, by which the whole tube containing the eye-piece can be moved horizontally, thus shifting the field of view so that any line of the spectrum can be brought to the side of the comparison lines. The instrument is mounted on a wooden base, grooved at the top to receive it. At the back side is a large knob by which the instrument is held when taking an observation. When directed to any

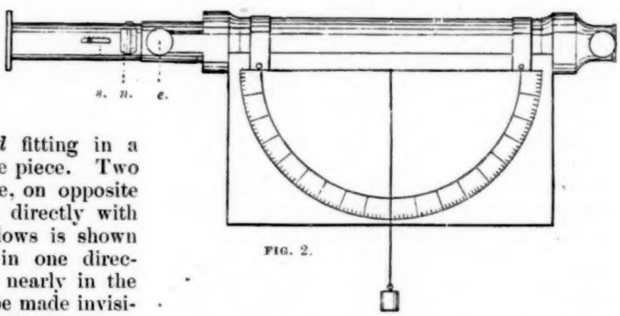


FIG. 2.

part of the sky, the altitude can be determined by means of the graduated circle and hanging weight shown in the figure.

Another device, much simpler, and of use,

[1] *Ann. phys. chim.*, xxiii. 274.
[2] *Comptes rendus*, lxxxix. 1033 and xci. 383.

it is believed, for general observation when less accuracy is required, is shown in fig. 3. A collar represented in section at *a* is inserted into the tube of the spectroscope, and fastened permanently so that its front side shall be just in the focus of the eye-piece. From the lower front edge to the upper back edge, a silk fibre passes, drawing back as it rises. The fibre will evidently appear in the field of view, as represented at *b*, as lines of diminishing intensity. A set of horizontal, equidistant spider-lines are attached to the front edge, hence just in focus. The line whose intensity is to be measured is made to appear parallel and near to one of the interference lines; and its intensity is expressed by the number of the spider-line at which the intensities correspond, counting downwards. And here it may be mentioned that such a scale of intensities (or, indeed, the scale afforded by the micrometer screw readings in the preceding apparatus) is not a scale of equal parts, a change of a unit in case of a line of high intensity being more than in case of a low intensity. This is, however, believed not to be a serious disadvantage in practice.

a b
Fig. 3.

The advantages of any practicable method of measurement over a mere estimation are evident enough. When estimated by the eye, it is believed to be impracticable to distinguish more than five grades of strength, while by this method quite fine shades of intensity can be measured; and what is, perhaps, of equal importance, measurements made against dark and light sky are apparently identical, a change in the brilliancy of the background affecting the appearance equally of the absorption and interference lines. Evidently an unaided estimation would very likely be at fault in such a case.

As to the accuracy actually attained in practice, it is found, in looking over the record of about a month past, that the whole range of the readings made at one observation, in ordinarily favorable weather, averages 0.3 of a revolution of the micrometer screw; and, as from four to twelve or more readings are always taken, according to the amount of variation noted, the probable error of the mean may be considered as about 0.03, as computation has shown in a number of cases. Now, as the whole range of the instrument used is from 1.0 to 5.7, it is evident that many grades of intensity are capable of appreciation. It is to be remembered, that these readings are purposely made in various quarters of the sky, so that discrepancies in readings are partly due to want of uniformity in the hygrometric state of the atmosphere. It should be stated, also, that such accuracy is not attainable below 2.0, as the value of a unit is then considerably less than above that value.

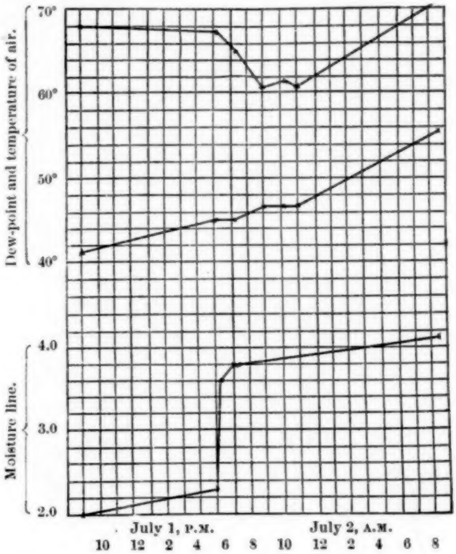
The regular record made at the observatory is as follows: The ordinary meteorological record is made three times daily. With the spectroscope, at least three sets of readings are taken, comprising measurements of the intensity of the moisture line at the horizon, at altitudes of 10°, 20°, 30°, and 90°. In all cases, the readings are taken in all quarters of the sky where there is sufficient light. A set of readings is also taken by setting the micrometer at 2.0, giving a faint line just visible in dark weather, and then measuring the altitude at which the moisture line is of the same strength. Such readings of altitude rarely vary more than 2° to 4° in settled weather. The strength of line and of the 'rain-band' is also estimated by the eye at each observation. At the same time the readings of the wet and dry bulb hygrometer are taken, as well as of a Regnault's condensing hygrometer. The wet and dry bulb hygrometer can be ventilated by means of a bellows, as suggested by Mr. H. A. Hazen, in a recent number of Science. Notes are made of the direction and velocity of the wind, of the clouds, and condition of the air.

One of the most interesting of the powers of the spectroscope thus used is its ability to detect relatively moist tracts in the atmosphere. While in settled weather entire uniformity at all points of the horizon is generally noted, in unsettled weather considerable differences are often observed. An excellent example of this power of the instrument occurred on May 26. During the morning when the observations were made, the air was very clear and dry, the moisture line therefore weak. At 6 A.M., measurements made entirely around the horizon showed that the line became invisible very uniformly at an altitude of 10°, except for about 45° of the north-eastern horizon, where the altitude of disappearance was 20°, while the intensity of the line at the horizon here was about double that elsewhere. There was no wind blowing, and no clouds of any kind were visible except a few wisps of cirrus cloud high in the east. These facts were all noted in the record at the time. At seven o'clock, when the next readings were taken, to my surprise this moist tract was found to be nearly filled up with a bank of stratus cloud, with no other clouds visible. At the same

time a similar moist region, of 15° altitude and perhaps 35° length, was discovered in the south-east; and this in turn was found fifteen minutes later to be partly filled with cloud. After an hour or so they had all disappeared. The appearance was as though a body of air heavily charged with moisture, having become heated, was seen rising bodily at six o'clock, while at seven the consequent cooling had condensed in part its moisture.

One of the most striking facts noted is the suddenness with which a hygrometric change occurs, as indicated by the spectroscope. During the fine weather of June 30 and July 1, the spectroscope had indicated unusually dry air with almost absolute uniformity. During July 1, as the diagram shows, there had been a very slight increase in the moisture present, as indicated by an observation at six P.M. Fifteen minutes later, happening to glance through the spectroscope, I was greatly surprised to see how much blacker the line looked. A new set of readings was taken, giving a much higher amount of moisture, as indicated by the sudden rise in the curve. The sky was almost entirely free from clouds, with a light breeze from the south-west. Measurements were made in both cases all along the western half of the horizon, the eastern being too dark at that hour. At seven o'clock a moderately dense bank of stratus clouds had risen in the west to an altitude of 15° or 20°. The record at seven and seven-thirty showed little further hygrometric change. The sky was soon entirely overcast with clouds. This hygrometric change was not a mere momentary one, connected with cloud-formation; but the later record showed it to be the beginning of a period of moist air and showery weather. The hygrometer, it will be noted in the diagram, gave little sign of change for some hours. Other sudden changes of equally striking character have been observed. That, as has been suggested by Capron and others, the physical state of the suspended water, the size of the aqueous particles, may have an influence in its light-absorbing power, and so explain in part such changes, is very possible; but the evidence that such is the case appears to be far from conclusive.

It is believed that a series of spectroscopic observations, continued for a considerable period of time at different stations, would give much light on a number of important questions in meteorology, particularly in the study of the formation of showers and storms. The instrument is apparently admirably adapted to do this work, by its ability to trace accurately the motions of masses of vapor in the upper atmosphere. The discussion of the more important questions which arise in carry-



ing on this investigation is deferred until a larger mass of figures and facts have been accumulated. C. S. COOK.

NOTES ON SASSAFRAS-LEAVES.

THERE are three distinct forms of sassafras-leaves. The simplest is ovate, varying to oval and obovate. A second form is three-lobed, the incisions running from near the middle of the upper half of the leaf's edge to the centre of the blade. The third form is midway between the entire and three-lobed sorts, and has but one side-lobe; the opposite half of the leaf being entire. It is as if one-half of a three-lobed leaf were joined by the midrib to the opposite half of an entire one of the same size. This form may be very appropriately called the 'mitten.'

In the study of these three forms, branches of sassafras have been gathered from a large number of places through the surrounding country. Some have been obtained from the woods, and others from the open field. Branches were cut from the largest trees and from the smallest, from vigorous trees and those of slow growth. Ten hundred and fifty leaves were examined: and of these, five hundred and thirteen were entire; four hundred


4
1
2
3

and fifty-eight, three-lobed; and seventy-nine, 'mitten form.'

The first leaves of spring were invariably entire, and a lobed leaf was rarely found until the fourth leaf was passed in counting from the base of the branch toward the tip. No regular order was discovered. In one case the arrangement was as follows: three entire, four three-lobed, one 'mitten,' one three-lobed, one 'mitten,' one three-lobed, one 'mitten,' one three-lobed; on another branch, four entire, one 'mitten,' five three-lobed, one 'mitten,' three three-lobed, three 'mittens.' The leaves on short spurs of old trees were nearly all small and entire; when the branches were somewhat longer, and the leaves larger, there were one or more three-lobed or 'mitten' leaves in the middle of the stem. A number of branches taken from slow-growing trees gave the following aggregate: entire leaves, seventy; 'mittens,' six; three-lobed leaves, three. A vigorous young sprout gave twenty-seven three-lobed leaves, one 'mitten' near the middle of the stem, and no entire leaves. Another had two entire blades at the base, and twelve three-lobed leaves above. A number of these rapidly-growing young trees together gave twenty-seven entire leaves, fourteen 'mittens,' and eighty-one three-lobed leaves.

The entire and smaller leaves are in the majority on slowly-growing trees; while, on the young sprouts, larger three-lobed leaves predominate. The 'mitten' form is mostly found with the entire leaves. This form of leaf is probably about equally divided between the 'right-handed' and 'left-handed;' though, of the number found (seventy-nine), those with the 'thumb' to the left, when held with under side upward, exceeded the other sort by half. About every thirteenth leaf is a 'mitten,' — a form not found mentioned in the botanical description of the sassafras.

There seems to be no order in the arrangement of the three forms upon the branch. Leaves from the buds were examined, and all of the three forms were found. Each kind is distinct, from a very early state; and there is no indication that one ever passes into the other. No intermediate forms have been found. The venation of the three forms is very much the same. There is a midrib running lengthwise through the leaf, and a strong lateral vein on each side, which runs from near the base to beyond the middle of the leaf. Smaller veins form the framework of the middle and upper parts of the leaf. The portion of parenchyma absent in a lobed leaf is midway between the strong lateral veins. This is very clearly shown in a 'mitten,' where one side is lobed, and the other entire. It would seem as if the lobing is a failure to fill up the framework, and apparently due to a too vigorous growth of the veins, and a lack of a sufficient amount of the soft, filling tissue. In the formation of leaves the sassafras is certainly 'at loose ends,' but in this it is not alone.

Fig. 1 shows an entire sassafras-leaf; fig. 2, a three-lobed leaf; and fig. 3, a 'mitten.' Fig. 4 shows the young leaves of the three forms. All the illustrations are drawn from nature. Byron D. Halsted.

New York, July 2, 1883.

THE UNITS OF MASS AND FORCE.

In the original definition of the gram it was regarded as a weight, and therefore a force, being the weight at the level of the sea, and at the latitude of 45°, of one cubic centimetre of water at its maximum density. It was thus virtually defined as a force. But as we shall soon see, although defined as a unit of force, it has become in practice a unit of mass. In the C. G. S. system of units this change is accepted, and the definition is modified accordingly; that is, one cubic centimetre of water is taken as the unit of mass, and this mass is called the gram without reference to its weight.

In volume i., *Cours de physique*, M. Jamin criticises this change. The high standing and character of this great work, as well as the eminence of its author, entitle his views to respectful consideration, especially as the question involves the fundamental elementary conceptions of physics in a way to render it of interest to the general student.

We set out with the proposition that what we commonly consider units of weight, such as the *kilogram* and *pound*, practically become units of mass in all the ordinary affairs of life. The reason is, that in practice bodies are weighed by balancing them against pieces of metal, and not by means of a spring balance. A pound weight is indeed heavier the farther north we go; but then, whatever we weigh with it is heavier in the same ratio. Accordingly, if by means of a weight we weigh a pound of tea at the equator, at the poles it will still weigh the same as a pound weight, although in reality heavier than at the equator. This is obviously a great practical and commercial convenience; because the quantity or mass of the tea is the important question to those who deal in it, while its gravitating force is of secondary importance. Were a perfect

spring balance used which measured absolute weight, the dealer who should purchase tea at one latitude, and sell it at another, would be subject to a gain or loss, depending upon the difference in the force of gravity.

It is not, however, on merely commercial grounds that the change rests. For scientific purposes a unit is used as a term of comparison between different quantities of the same kind, and must be so defined and chosen as to fulfil this function with the greatest convenience. Now, a unit of force which shall furnish a direct and convenient standard of comparison between forces or weights at different places is entirely impracticable. At any one place the weight of a given mass of metal may be taken as a convenient unit; but this unit will change when we go to any other place, owing to the difference in the force of gravity. Indeed, every student of physics knows that the measure of the force of gravity at any one place is one of the most delicate and difficult problems in physics. In the definition which refers to the latitude of 45° it is assumed that the force of gravity is the same at all points on this parallel. We now know that this is not the case, and that if we adopt such a unit we shall have to define the exact spot on the earth's surface which is taken as the standard. Reference to such a standard would be impracticable. Hence a unit of force must be subsidiary to the unit of mass. The most convenient way of fixing it is to take the unit of mass as known, and to determine the force of gravity at the place of observation. The combination of the two gives a standard by which weight may be expressed in force. To be more explicit: if we have a piece of metal the mass of which we know to be one gram, and if we determine the force of gravity at the place to be n, then the gravitating force of that piece of metal will be known to be n units of force. In practice this must be the method used in physics, if an accurate measure of forces is really required.

Let us now consider M. Jamin's objections. He says that the mass of a body is not susceptible of direct determination; for to measure it we must commence by determining its weight in a balance, and afterwards dividing by the number which expresses the acceleration of gravity at the latitude of 45° and at the level of the sea. It is difficult to attribute this remark to any thing but inadvertence, since the division by g at 45° is necessary only on the French system. If we measure it by means of a balance having grams as weights, the resulting weight is at once the mass on the C. G. S. system, no matter where the weighing is made, and therefore needs no division whatever.

He then adds, "Suppose, on the contrary, that we have to measure a force: we determine it directly by means of weights at the place of observation. Afterwards we apply to these weights the corrections relative to the latitude and the altitude, to have an expression of the force as the function of a normal gram. We must remark that we cannot avoid these corrections to taking mass as the fundamental unit; because it is always weights that we measure, and the course followed in the experiments is necessitated by the nature of things." This is quite true, but it does not prove that one system affords any more convenient unit of force than the other. SIMON NEWCOMB.

STANDARD RAILWAY TIME.

THE problem of simplifying the system of time standards used by the railways of this country seems to be near solution. The representatives of various railway-lines, who are to-day in session at Chicago, will receive the report of the secretary, Mr. W. F. Allen, and, it is expected, will take final action. For some years past, committees of various scientific bodies, as the American metrological society, the American association for the advancement of science, and the American society of civil engineers, have called attention to the urgent need of reform in the standards of time in use, and suggested plans for action. The railways, which are naturally most interested in the movement, have recently taken hold of the matter in earnest. The plan which has met with the most favor is that in which five standards of time, differing by consecutive hours, are proposed for the whole territory occupied by the United States and Canada. These are based upon the meridians from Greenwich, but receive other names for purposes of convenience. It is proposed by the railways that in Canada the standard shall be known as *intercolonial time*, and shall coincide with the local time on the meridian four hours, or 60°, west of Greenwich. In the United States the standards will be known as *eastern*, *central*, *mountain*, and *Pacific time*, and coincide with the local times on the meridians five, six, seven, and eight hours, or 75°, 90°, 105°, 120°, respectively, west of Greenwich. The advantage of this system is, that the standards will differ from the true local times of the various parts of the country by amounts not greater than thirty minutes, if the divisions are made rigidly according to longitude, and no one will be inconvenienced

thereby. The great difficulty, however, of the plan, lies in the selection of the places where the changes of one hour are to be made; and as some of these, especially that between eastern and central time, must pass through country well settled, no matter how much freedom is allowed in selecting the points of change, it has seemed to many that the inconvenience would be great. Railway interests require that the changes be made at the termini of sections of the road, which are often large cities. At these points there will be two times, — one for eastern, one for western roads, differing by an hour. In dealing with this practical difficulty, the railways have shown a desire to conform as nearly as possible to the theoretical system, but have adopted the principles that "changes from one standard to another should be made at well-known points of departure," and that "these changes should be made at the termini of roads, where changes now occur, except on the transcontinental lines and in a few other unavoidable cases, where they can be made at the ends of divisions."

At the railway-time conventions held in St. Louis and New-York City in April last, the following resolutions were adopted: —

1°. That all roads now using Boston, New York, Philadelphia, Baltimore, Toronto, Hamilton, or Washington time as standard, or standards based upon meridians east of those points, or adjacent thereto, shall be governed by the seventy-fifth meridian or 'eastern time' (four minutes slower than New-York time).

2°. That all roads now using Columbus, Savannah, Atlanta, Cincinnati, Louisville, Indianapolis, Chicago, Jefferson City, St. Paul, or Kansas City time, or standards based upon meridians adjacent thereto, shall be run by the ninetieth meridian time, to be called 'central time' (one hour slower than 'eastern time,' and nine minutes slower than Chicago time).

3°. That west of the above-named section the roads shall be run by the one hundred and fifth and the one hundred and twentieth meridian times respectively (two and three hours slower than 'eastern time').

4°. That all changes from one hour standard to another shall be made at the termini of roads or at the ends of divisions.

Another resolution provided that the secretary should prepare a pamphlet containing an explanation of the subject, with accompanying maps, and endeavor to secure the acquiescence of all parties to the proposed plan, that the next convention might take final action.

The report of the secretary contains a fine railway-map, with the standards proposed for each road designated by different colors. It is the intention to use the eastern standard from Maine and the eastern coast to Detroit, Mich., and Bristol, Tenn.; but all the Ohio and Georgia railways will use the central standard, as well as those in Pennsylvania west of Pittsburg. The western railways whose termini are in Buffalo, Salamanca, and Charlotte, are allowed to use the central standard as far east as those points. The important places where the change of one hour from eastern to central time occurs, are Detroit, Buffalo, Pittsburg, Charlotte, and Augusta. The change from central to mountain time is made at Bismarck, North Platte, Wallace, Coolidge, and others; from mountain to Pacific time, at Ogden, Yuma, and others.

The secretary, Mr. Allen, has received assurances from the great majority of roads, that the system is approved. At the beginning of this month, railways operating 70,000 miles of road had responded favorably; and replies were coming in daily, none in the United States having refused assent. The roads centring in Boston gave assent, provided satisfactory arrangements could be made with the Cambridge observatory, upon which they depend for their time-signals. Of this there can be no doubt, as it may be assumed that every observatory in the country will contribute its part in the movement which inaugurates such a needed reform. The eastern standard differs from Boston time by sixteen minutes.

It seems almost certain, then, that the convention now in session will authorize the proposed change, and appoint a time when the plan shall be put into practical operation. On that date the observatories will make the change in their signals which the railways use, and the system will at once be under trial. The next question will be, whether the cities will adopt the railway system for their use. Of this there can be little doubt; and, in cases where two standards differing by an hour come together, it will be necessary to adopt one of the two for the city standard. The state of Connecticut, which several years ago hastily adopted New-York time for the standard, will have the small change of four minutes to authorize. All these adjustments may be left to the future. They will be made or not, as the popular interests demand. Of the wisdom of the action of the railway managers there can be no doubt. Without discussing the relative merits of the plan adopted, and others which have been suggested, it is certain that the present confused arrangement should be abolished. The new plan is simple

and practicable; and its adoption is an important reform, which is deserving of hearty support and encouragement.

LETTERS TO THE EDITOR.

Phalansterium digitatum Stein.

THERE is no published evidence that the infusorial colony here referred to has been seen by any observer except its German discoverer. It is stated not to occur in English waters; and this uncommon animalcule had not been taken in America, until the writer recently found it in considerable profusion, attached to the leaflets of Myriophyllum from a millpond near this city. The colonies and the enclosed zoöids differ from their German relatives in no essential character, the only perceptible divergence being in the somewhat smaller size of the American Infusorium.

The tubular colonies, which take an irregular digit-like form, and branch somewhat dichotomously, are in great part built up of granular digestive rejectamenta remarkable for their coarseness. The distal extremity of each tubule is slightly inflated, each zoöid sitting singly in the hollow thus formed, except after having undergone the reproductive process, when two or more may be present, the flagellum alone extending beyond the aperture.

The conical collar, embracing the flagellum for some distance above its point of origin, is often thickened by an outward flow of the body-sarcode; but whether a regular circulation takes place in the collar substance could not be determined.

Although the zoöids are apparently entirely free from all connection with the walls of the zoocytium, they have the power of suddenly darting back into the tubules for a distance equal to two or three times their length. They seem to exercise this accomplishment at pleasure, but especially when any unwelcome object comes in contact with the flagellum. I have seen a large animalcule glide across the front of a colony, and each zoöid in regular succession, as its flagellum was touched, shoot back into the tube, remaining there some minutes before cautiously reapproaching the aperture.

I have several times witnessed the reproductive process, and have verified the statement that it takes place by transverse fission. An interesting fact in this connection is, that the only other species of the genus reproduces itself by dividing longitudinally, a method directly the opposite of that which obtains with the present form.

The two posteriorly located contractile vesicles pulsate at intervals of about thirty seconds.

DR. ALFRED C. STOKES.

Trenton, N.J.

Solar constant.

I enclose a translation of a portion of a letter to me from Dr. Josef Pernter of the Austrian meteorological service. Dr. Pernter writes: —

"Speaking of radiation, I remember to have read several times in SCIENCE, under the 'letters to the editor,' various things concerning the solar constant, — lately, a letter from John LeConte, but which, like former communications, appears to make the subject a little unclear.

"The solar constant is a quantity of heat, and the number which is the expression for the solar constant must mean calores. If, for example, Violle says the solar constant is 2.54, then it must be 2.54 calores. But since the solar radiation is a summation, during time, extending over space, the duration and the surface certainly come into the question. The minute has been taken as the unit of time, and the square centimetre as the unit of space.

"That the solar constant is 2.54 calores, means, therefore, that the sun's rays bring to the outside of our atmosphere, in each minute, 2.54 heat-units upon each square centimetre. What becomes of these heat-units, or calores, does not belong at all to the conception of the solar constant.

"The new solar constant of Langley, 2.84, signifies, consequently, that the amount of heat furnished per minute per square centimetre by solar radiation is 2.84 calores. But this number, 2.84 calores, must be comprehended. Lately the term 'calore' has been used in two significations, — the large calore, or the amount of heat that raises one kilogram of water 1°; and the small calore, or the amount of heat which raises a gram of water 1°. The latter, or small calore, is applied to the solar constant. Expressed in large calores, the solar constant of Langley would not be 2.84, but .00284 calores; that is, 1,000 times smaller.

"After these explanations, one can immediately say how many great or small calores fall upon the square metre per minute from the solar radiation; viz., 10,000 times as many as on the square centimetre."

FRANK WALDO.

Deutsche seewarte, Hamburg, Germany, Sept. 16, 1883.

Dissemination of Phlox.

I have had for some time past, on my table, some capsules of Phlox Drummondii, which is so commonly cultivated in gardens. The capsules were picked while still green, and had dried gradually. Several times I have been puzzled at finding small seeds and parts of the capsule of a plant on the table, and could not think where they came from; but, a day or so since, I heard a sharp pop, and, looking up, saw that one of the capsules had burst, and sent the seed several feet away. Since then it has often occurred. This is an evident means for the dissemination of the seed. The most of the capsules I have examined have perfected only one seed, instead of three; and the sudden opening of the capsules have sent the seeds flying far and wide.

JOS. F. JAMES.

Cincinnati, O.

The Iroquois institutions and language.

The very courteous and complimentary manner in which my work on the Iroquois book of rites has been noticed in a recent number of this journal has made me reluctant to take exception to any portion of the review. On further consideration, however, I must beg to be allowed, in the interests of both science and history, to refer to one or two of the remarks of my friendly critic. He expresses the opinion that 'the sceptical reader' may be inclined to regard the portion of the work which relates to 'the league and its founders' rather as 'classic historical romance' than as history; and this on the sole ground (as I understand his suggestion) that the Iroquois cannot be supposed to have been capable, five hundred years ago, of the intellectual efforts implied in this narrative. This suggestion, it will be seen, opens up the entire question of the comparative mental capacity of civilized and uncivilized, or rather unlettered, races.

The question is one altogether too large to be fully discussed in this place. But as regards the particular subject now referred to, I may remark that the existence of the league itself, with all its judicious and statesmanlike regulations, is a fact of which there can be no possible question. Any one can see this remarkable constitution in full and vigorous operation among the three thousand Iroquois on their Canadian reservation. There is ample evidence to show that this league existed in its present form when the people who maintained it first became known to European explorers. It is clear, therefore, that whatever intellectual power was needed for its formation was possessed by the Iroquois before they acquired any tincture of foreign civilization.

But why should their capacity for forming such a government be questioned? The Iroquois tribes, when

first known to Europeans, and doubtless for centuries before that time, were in a social stage at least as far advanced as that of our German ancestors in the days of Tacitus. We know that these barbarians, if we choose so to style them, had evolved a regular system of government, combining very ingeniously the methods of democracy and aristocracy, and comprising the germs of the English constitution. On this point the often-cited passage of Montesquieu will bear to be requoted and emphasized. "In perusing," writes the great legist, "the admirable treatise of Tacitus 'On the customs of the Germans,' we find it is from that nation the English have borrowed the idea of their political government. *This beautiful system was invented first in the woods.*" Will any one reply that the German barbarians, being of the Aryan stock, must be supposed capable of intellectual achievements which barbarians of the Indian race could not be expected to compass? I think the able and liberal-minded reviewer will agree with me, that reasoning of this 'high priori' sort, which assumes the very point in question, would be any thing but logical or satisfactory.

The reviewer is kind enough to say that many of the chapters in my volume "indicate immense research, and are of great value both ethnologically and philologically." I can assure him that equal diligence was exercised in preparing the chapters on the league and its founders, and I know of no reason why they should be deemed less accurate or less valuable. In these, moreover, as well as for the other portions of the work, I have been careful to indicate the sources of my information. Nothing will be easier than for any one who has doubts as to its correctness to repeat my inquiries, and to satisfy himself on that point. But I am happy to say that the communications which reach me from many quarters seem to show that no such doubts are likely to be entertained; at least, by any well-informed persons. Writers of the highest authority on American and Indian history receive the statements of the book as entirely authentic, and speak of it in terms too flattering for me to repeat.

Let me conclude by expressing the pleasure with which I have learned from this review that the valuable work of the excellent and indefatigable missionary-linguist, the late Father Marcoux, on the Iroquois language, is about to be published by the Bureau of ethnology. The idioms of the Huron-Iroquois group stand, perhaps, at the head of the best-known Indian languages as subjects of philosophical study. It is doubtful if even the Quichua or the Aztec equals them in comprehensive force, or in subtlety of distinctions. More than two centuries ago the learned missionary Brebeuf was struck with the resemblance of the Huron to the Greek; and in our own day Professor Max Müller, after a careful study of the Mohawk tongue, has expressed the opinion that the people who wrought out such a language 'were powerful reasoners and accurate classifiers.' The works of M. Marcoux, in conjunction with those of his distinguished pupil and successor, M. Cuoq, will afford ample means for the study of one, and perhaps the finest, of this remarkable group of languages.

In connection with this subject, it is proper to refer to the doubt expressed by the reviewer as to the correctness of the linguistic works of the French missionaries. It is suggested that they have made mistakes in grammar, and in particular that they have not been able to distinguish between the feminine and the indeterminate inflections. Now, it must be remembered that the intelligent and well-educated missionaries, whose competency is thus questioned, have for many years spoken and written the Iroquois language almost as familiarly as their native speech, and have published many books in that language for the use of their converts. Their predecessors, whose experience they have inherited, had been engaged in the same work for more than two hundred years. To suppose them so grossly ignorant of the grammar of the language as is now suggested is much the same as supposing a professor of Latin in an English or American college to be unable to distinguish between the genitive and the accusative cases in that language. If the work of Marcoux is so erroneous, it is clearly unfit to be published in a national series like that of the Ethnological bureau. In justice both to the missionaries and the bureau, I am glad to be able to show, by the best possible evidence, that the suspected errors do not exist. The Iroquois must be supposed to know their own language. The text of their Book of rites, fortunately, presents a test which is conclusive. In preparing the translation of this text, with the aid of the best native interpreters, I had occasion, as the appended glossary shows, to make constant use of the publications of M. Cuoq on the Iroquois tongue, and found them invariably correct. In particular, I may mention, the indeterminate form frequently occurs, employed precisely as indicated by him. The bureau may therefore safely add the work of M. Marcoux to the other valuable publications which have done so much credit to the scholarship of their authors and to the liberality of the government.

H. HALE.

THOMSON AND TAIT'S NATURAL PHILOSOPHY. — I.

A treatise on natural philosophy. By Sir WILLIAM THOMSON LL.D., D.C.L., F.R.S., and P. G. TAIT, M.A. Vol. i., part ii., new edition. Cambridge, *University press*, 1883. 25+527 p. 8°.

THE first edition of vol. i. (23+727 p.) of this work was published by the delegates of the Clarendon press at Oxford, 1867. The authors then intended, as appears from their preface, to complete the work in four volumes. The remaining three volumes have, however, never appeared, much to the regret of all students of mathematical physics; and the authors state that the "intention of proceeding with the other volumes is now definitely abandoned."

In 1879 a new and enlarged edition was published of a portion of vol. i., entitled part i. (17+508 p.), including that part of the first edition contained in the first 336 pages; and now we have the remainder of vol. i., entitled part ii., which has been enlarged by important additions from 390 to 527 pages.

At p. 22 will be found a schedule of the alterations and additions in part i., and, at p. 24, those of part ii. "The most important part of the labor of editing part ii. has been borne by Mr. G. H. Darwin," whose remarkable papers in the Philosophical transactions upon the mathematical physics of the earth,

past and present, have placed him in the front rank of the cultivators of that science. His contributions to part ii. are duly accredited to him in the above-mentioned schedule.

The original object of this treatise is stated to be twofold; viz., "to give a tolerably complete account of what is now known of natural philosophy, in language adapted to the non-mathematical reader, and to furnish to those who have the privilege which high mathematical acquirements confer, a connected outline of the analytical processes by which the greater part of that knowledge has been extended into regions as yet unexplored by experiment."

From the nature of the case, the success of the authors in the attainment of their first object was small, compared with the second; for in order to give an intelligible account, to one unaccustomed to mathematical reasoning, of the general tenor and results of such reasoning, requires not only capacities such as few mathematicians have had in our day, except Clifford, but requires, also, an amount of space incompatible with the second and principal object which the authors had in view. In order, however, better to reach the non-mathematical reader, the authors published a work entitled 'Elements of natural philosophy, part i.,' which was only an abridgment of this 'treatise,' made by simply omitting all the advanced mathematical developments.

The second and principal object, however, of the authors, was one in which they, perhaps, were better fitted to succeed than any who could be selected. Their object was a large one, and its attainment was undertaken in a large way. It involved the presentation of the general subject of kinematics, or the geometry of motion considered apart from the forces causing it, including the exposition and use of generalized co-ordinates; and the consideration of harmonic motion, which "naturally leads to Fourier's theorem, one of the most important of all analytical results as regards usefulness in physical science," and including, also, the higher parts of the analytical discussion of curves and surfaces in space, of three dimensions. Next it required an extended development of dynamical laws and principles founded on Newton's Principia, comprising the dynamics of a particle and of a rigid body, and the whole of what is now termed kinetics worked over and "developed from the grand basis of the conservation of energy." The scope of the work demanded, also, the establishment of the principal formulae of spherical harmonics, a branch of analysis whose character we shall explain more at length hereafter.

All these and other subjects, which are usually regarded as but distantly related to the subject in hand, form a necessary part of a work whose object is as wide as that proposed by the authors. But it is hardly too much to say, that every important theory treated has received at their hands, not only elucidation, but additions of importance.

In order to make this paper as useful as may be, it has seemed best, in what follows, to content ourselves with the attempt to give an account to mathematical readers of the more important developments contained in the work, and not to engage in the task of trying to make an elucidation of its contents suitable for the general reader.

When we come to consider in particular the contents of part ii., it is found to be upon the general subject of *statics;* though many subjects, such as elasticity, the tides, etc., not usually treated in works on that subject, are here included. It consists of three chapters, the first of which is but five pages in length, and is merely introductory. It states and illustrates the utter impossibility of submitting the *exact* conditions of any physical question to mathematical investigation by reason of our ignorance of the nature of matter and molecular forces, but shows that approximate solutions obtained by neglecting forces which do not affect the conclusions sought to be established, and by regarding bodies as rigid which are nearly so, lead to practically the same results, as to the equilibrium and motion of bodies, as we should be led to by the solution of the infinitely more transcendent problem which has regard to *all* the forces acting.

In case, however, we consider the bending or other deformations of bodies regarded as elastic, we make a second approximation to the exact treatment of physical questions; and, by introducing modifications of elasticity due to changes of temperature, we should make a third approximation, which might be carried one step farther by taking account of conduction of heat, and farther still by considering the modifications of ordinary conduction due to thermo-electric currents, etc. In view of all this, the authors say, "The object of the present division of this volume (i.e., part ii.) is to deal with the first and second of these approximations. In it we shall suppose all solids either *rigid* (i.e., unchangeable in form and volume) or *elastic;* but, in the latter case, we shall assume the law connecting a compression or a distortion with the force which causes it, to have a particular form deduced from experiment. . . . We shall also suppose fluids,

whether liquids or gases, to be either *compressible* or *incompressible*, according to certain known laws; and we shall omit considerations of fluid friction, although we admit the consideration of friction between solids."

The next chapter (v.) comprises pp. 6 to 100, and its especial object is set forth in the introductory section (454), as follows: "We naturally divide statics into two parts, — the equilibrium of a particle, and that of a rigid or elastic body or system of particles, whether solid or fluid. In a very few sections we shall dispose of the first of these parts, and the rest of this chapter will be devoted to a digression on the important subject of attraction." In other words, this chapter is devoted, with the exception of a couple of pages, to an extended treatment of attraction according to the law of the inverse square of the distance as applied to gravitation, electricity, and magnetism.

After a brief investigation of the usual formulae for the attraction of the spherical shell, circular disk, thin cylinder, circular arc, etc., the main subject of the chapter is reached, which is the modern mathematical theory of potential; which theory is the principal means now employed in the discussion of questions relating to the distribution of attracting matter, and the forces caused by it. This theory, due as it is to the analytical discoveries of Laplace, Green, Gauss, and others, might, nevertheless, have long remained comparatively barren of fruitful results in physics, had it not been for the genius of Faraday, who, though unskilled in the use of analysis, had a most powerful grasp of geometric and physical relations. In the words of another,[1] "Faraday, in his mind's eye, saw lines of force traversing all space, where mathematicians saw centres of force attracting at a distance; Faraday saw a medium where they saw nothing but a distance; Faraday sought the seat of the phenomena in real actions going on in the medium, they were satisfied that they found it in a power of action at a distance." He conceived of lines of gravitational force as holding the planets in their orbits. These lines radiated through all space from the attracting body as a nucleus, regardless of the existence or non-existence of bodies upon which the attraction could be exerted. Furthermore, Faraday thought of each attracting body as surrounded at different distances by successive level surfaces, — like that of the ocean, for example, or the upper limit of the atmosphere; which surfaces cut the lines of force everywhere at right angles. This was not only true of gravitating matter, but each electrified body also had its system of lines of electrical force, and its corresponding system of level surfaces; and each magnet had its magnetic system as well. The geometry of these lines and surfaces is the basis of Faraday's reasoning in his 'Experimental researches,' and is the geometric truth hidden in the analytic discoveries clustering around Laplace's, Poisson's, and Green's theorems.

[1] Preface of Maxwell's Electricity and magnetism.

That we may call these relations more clearly before the mind, consider for a moment the so-called 'equation of continuity' of an incompressible fluid; which equation is divined from the geometric truth, that the quantity of such a fluid, which flows into any assumed closed surface, taken entirely within it, is equal to that flowing out, or that the total *flow* is *nil*. This is precisely expressed by the equation

$$\int F\, d\, S = 0, \qquad (1)$$

in which $d\, S$ is the area of the element of the assumed closed surface, F is the normal flow per square unit at that element, and the limits of integration are so taken that it extends over the entire surface. There is also another form of the equation of continuity, expressing the kinematic truth, that, in an incompressible fluid, the variations of the component velocities in the directions x, y, z, balance; i.e., their algebraic sum is *nil*, which may be written thus: —

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0, \qquad (2)$$

in which u, v, w, are the component velocities in the directions x, y, z, respectively.

Now, it is not difficult to picture to the mind the motions occurring within the mass of an incompressible fluid; such as water, for example. In whatever way it may be moving, we can think of stream-lines along which the different parts of it flow. A number of these lines, side by side, can be taken to form a stream, and can be thought of as bounded by a kind of tubular surface; which surface might be regarded as the boundary of the stream, which isolates it from surrounding streams. If the stream has the same velocity at every point along the tube, then its cross-section must be uniform; but, where the velocity is less, the cross-section is proportionately increased, and *vice versa*. This follows from the fact that the same quantity must pass each cross-section per unit of time. A tube in which a unit of volume passes a given cross-section per unit of time is called a *unit-tube*. Now, the forces of attraction in free space, caused by any distribution of matter, electricity, or magnetism,

follow precisely the same laws as the velocities and flow of incompressible fluids; for, consider for the moment the lines of force starting from the surface of some attracting body (a magnet, for example). They gradually diverge as the distance increases, and curve away into space. Each one of these lines may be taken as the representative of a definite amount of attraction, which is the same at all points along it; and if a tubular surface be supposed to exist, including everywhere certain of these lines which lie beside each other, and no others, the total amount of force acting across every cross-section of the tube is the same: hence equations (1) and (2) apply as well to forces of attraction as to velocities of an incompressible fluid, provided F, u, v, w, be taken to be the component forces along the normal and along x, y, z, respectively, and provided that none of the attracting matter be contained within the closed surface considered in equation (1), or at the point considered in equation (2). In order to the farther development of these equations, let us compute the work which would be obtained in carrying a unit of attracted material from one given position to another. The work is found from the usual expression

$$V = - \int (u\,dx + v\,dy + w\,dz), \qquad (3)$$

in which u, v, w, being component forces, the limits of the integration are the co-ordinates of the two given points; but what path is taken between these points is of no consequence, because the amount of work depends alone upon their difference of level:

$$\therefore u = -\frac{dV}{dx},\ v = -\frac{dV}{dy},\ w = -\frac{dV}{dz}, \qquad (4)$$

in which the right-hand numbers are partial differential coefficients. V is evidently a function of the co-ordinates such that its value depends upon position, and not upon the kind of co-ordinates employed. The point which fixes the lower limit of the integral in (3) is usually taken at infinity; and the value of V taken between it and the point fixing the upper limit is called the *potential* of the latter point.

By help of (3), we may put equation (1) in the form

$$\int \frac{dV}{du}\, dS = 0, \qquad (5)$$

in which du is the element of the normal to the closed surface considered.

And by substituting in (2) the values given in (4), we have,

$$\frac{d^2 V}{dx^2} + \frac{d^2 V}{dy^2} + \frac{d^2 V}{dz^2} = 0, \qquad (6)$$

which is Laplace's equation, and is often written in the abbreviated form, $\nabla^2 V = 0$. Poisson showed, that, when the point at which the potential is to be computed is within the mass of the attracting matter, the right-hand member of (6) should no longer be *nil*, but $4\pi\rho$ instead, in which ρ is the density of the matter at that point. Similarly, the right-hand member of (5) becomes $4\pi m$ when an amount of matter m is included within the closed surface considered.

Equation (6) states that V must be such a function of the co-ordinates, that, if we take its three partial second differential coefficients and add them, their sum is *nil*. What possible algebraic forms are there which fulfil this condition? They are, of course, to be found by attempting to solve the differential equation (6). But it is to be seen beforehand, from the manner in which that equation was established, that it must have an infinite number of solutions; for V must be such a function as to be capable of expressing the work to be obtained from a unit of attracted matter when brought from infinity into the presence of attracting matter, whatever its distribution in space. The function V must therefore, in general, be different for every different distribution of attracting matter.

The integration of equation (6), and the discussion of its various solutions, constitute the branch of mathematics called spherical harmonic analysis; and to it the authors have devoted pp. 171 to 219, in part i. The formulae there obtained are employed, whenever required in the present chapter, to express the potential, or the attraction of matter distributed according to laws not conveniently to be treated by less elementary methods.

As the study of spherical harmonics has been comparatively neglected in this country, a short digression, explaining some of their properties, may be useful.

From the nature of attraction, it being toward fixed centres, it appears that polar co-ordinates would be more suitable to express its relations than rectangular co-ordinates; and, in fact, equation (6) is usually transformed to polar co-ordinates in space before integration, which co-ordinates may be taken to be the radius vector, the latitude, and the longitude of the point at which the potential is computed.

It may be shown that there are two general forms of solution of this polar differential equation, — one in ascending powers of the radius vector; and the other in ascending powers of its reciprocal, with coefficients depending upon sines or cosines of the angular co-ordinates.

As these series may be broken off at any point by the vanishing of the arbitrary numerical coefficients introduced during integration, these solutions may be in terms of the radius vector of any degree, positive or negative.

It is then found that a most important and simple class of solutions, called zonal harmonics, is those which are independent of the longitude, and consequently contain but two variables, — the radius vector and the latitude.

If in any harmonic we assume some special value of the radius vector for consideration, we evidently confine our attention to a spherical surface; and the expression is then spoken of as a surface harmonic, in distinction from that in which the radius vector is a variable, in which case it is called a solid harmonic.

On the surface of a sphere of given radius, it is possible to suppose the values of a surface-harmonic to be laid off graphically along the radii to each point, toward or away from the centre, according to their sign. This will give a picture to the mind of the distribution of the surface-harmonic.

Now, in a zonal harmonic of the first positive degree (which varies as the sine of the latitude) the surface-distribution is all positive on one side of the equator, and all negative on the other. A simple zonal harmonic of the second degree has a distribution like that included between a nearly spherical ellipsoid of revolution about the polar axis and a sphere when the two intersect along two parallels of latitude. The ellipsoid may be prolate or oblate. The number of zones depends, in any case, upon the degree of the zonal harmonic, and is such that the number of parallels of latitude at which the distribution changes sign is the same as the degree; and they are symmetrically situated about the equator, so that in the odd degrees the equator is itself such a parallel.

There are other solutions, called sectorial harmonics, in which the surface-distribution changes sign at equidistant meridians, and other solutions still, which are a combination of these two, called tesseral harmonics, in which the sign of the distribution changes, checkerboard fashion, at parallels and meridians. The sectorial harmonics are, however, in reality, nothing more than the combination of a number of zonal harmonics of the same degree, whose poles are situated at equal distances along the equator; and the tesseral harmonics are combinations of the sectorial with the zonal harmonics. Indeed, the most general harmonic is one by means of which any surface-distribution whatever may be expressed by properly determining the constant coefficients, and is merely a combination of zonal harmonics superposed one upon another, with poles situated in some irregular manner upon the surface of the sphere. This brings us to the fundamental theorem stated in section 537, upon which the special importance and usefulness of these functions rest, — "A spherical harmonic distribution of density (i.e., matter) on a spherical surface produces a similar and similarly placed spherical harmonic distribution of potential over every concentric spherical surface through space, external and internal; and so, also, consequently, of radial component force. . . . The potential is, of course, a solid harmonic through space, both external and internal; and is of positive degree in the internal, and of negative degree in the external space," as is evidently necessary, if the series expressing the potential in these two cases are to converge. When we come to treat in the same equation the potentials of a given point due to two different bodies, or systems of bodies, a remarkable relation is found to exist between them, called, from its discoverer, Green's theorem, which, though somewhat complicated when expressed in rectangular co-ordinates, has been put by Maxwell in a simple form, which may be written

$$\int V_1 dm_2 = \int V_2 dm_1, \qquad (7)$$

in which the subscripts refer to the first and second systems respectively, and the integrations are to be extended so as to include the total masses m_1 and m_2 respectively of the two systems. Laplace's and Poisson's equations are, of course, particular cases of Green's theorem. Thomson has effected an important extension of Green's theorem, given on pp. 167 to 171, part i. Constant references are made to these theorems, not only as to their direct application, as we have presented it, but in their application to the inverse question of determining what the distribution of matter must be to produce a given distribution of potential.

The most extended and important application of the theories of attraction and potential treated in this chapter is that of ellipsoids and ellipsoidal shells, — a subject which is closely connected with that of the figure of the earth, and one which has engaged the prolonged attention of many of the most powerful mathematical intellects of the past. A full account of the course of discovery in this field is found in Todhunter's History of the theories of attraction and figure of the earth, 2 vols.

Ten pages of new matter (pp. 40–50) have

been inserted in this edition, embracing modern investigations of importance on this subject.

(To be continued.)

OBLIGATIONS OF MATHEMATICS TO PHILOSOPHY, AND TO QUESTIONS OF COMMON LIFE.[1] — II.

I SAID that I would speak to you, not of the utility of the mathematics in any of the questions of common life or of physical science, but rather of the obligations of mathematics to these different subjects. The consideration which thus presents itself is, in a great measure, that of the history of the development of the different branches of mathematical science in connection with the older physical sciences, — astronomy and mechanics. The mathematical theory is, in the first instance, suggested by some question of common life or of physical science, is pursued and studied quite independently thereof, and perhaps, after a long interval, comes in contact with it, or with quite a different question. Geometry and algebra must, I think, be considered as each of them originating in connection with objects or questions of common life, — geometry, notwithstanding its name, hardly in the measurement of land, but rather from the contemplation of such forms as the straight line, the circle, the ball, the top (or sugar-loaf). The Greek geometers appropriated for the geometrical forms corresponding to the last two of these the words *σφαῖρα* and *κῶνος*, our sphere and cone; and they extended the word 'cone' to mean the complete figure obtained by producing the straight lines of the surface both ways indefinitely. And so algebra would seem to have arisen from the sort of easy puzzles in regard to numbers which may be made, either in the picturesque forms of the Bija-Ganita, with its maiden with the beautiful locks, and its swarms of bees amid the fragant blossoms, and the one queen-bee left humming around the lotus-flower; or in the more prosaic form in which a student has presented to him in a modern text-book a problem leading to a simple equation.

The Greek geometry may be regarded as beginning with Plato (B.C. 430–347). The notions of geometrical analysis, loci, and the conic sections, are attributed to him; and there are in his 'Dialogues' many very interesting allusions to mathematical questions, — in particular the passage in the 'Theaetetus' where he affirms the incommensurability of the sides of certain squares. But the earliest extant writings are those of Euclid (B.C. 285). There is hardly any thing in mathematics more beautiful than his wondrous fifth book; and he has also, in the seventh, eighth, ninth, and tenth books, fully and ably developed the first principles of the theory of numbers, including the theory of incommensurables. We have next Apollonius (about B.C. 247) and Archimedes (B.C. 287–212), both geometers of the highest merit, and the latter of them the founder of the science of statics (including therein hydrostatics). His dictum about the lever, his 'Εὕρηκα,' and the story of the defence of Syracuse, are well known. Following these we have a worthy series of names, including the astronomers Hipparchus (B.C. 150) and Ptolemy (A.D. 125), and ending, say, with Pappus (A.D. 400), but continued by their Arabian commentators, and the Italian and other European geometers of the sixteenth century and later, who pursued the Greek geometry.

The Greek arithmetic was, from the want of a proper notation, singularly cumbrous and difficult; and it was, for astronomical purposes, superseded by the sexagesimal arithmetic, attributed to Ptolemy, but probably known before his time. The use of the present so-called Arabic figures became general among Arabian writers on arithmetic and astronomy about the middle of the tenth century, but it was not introduced into Europe until about two centuries later. Algebra, among the Greeks, is represented almost exclusively by the treatise of Diophantus (A.D. 150), — in fact, a work on the theory of numbers, containing questions relating to square and cube numbers, and other properties of numbers, with their solutions. This has no historical connection with the later algebra introduced into Italy from the east by Leonardi Bonacci of Pisa (A.D. 1202–1208), and successfully cultivated in the fifteenth and sixteenth centuries by Lucas Paciolus, or de Burgo, Tartaglia, Cardan, and Ferrari. Later on, we have Vieta (1540–1603), Harriot, already referred to, Wallis, and others.

Astronomy is, of course, intimately connected with geometry. The most simple facts of observation of the heavenly bodies can only be *stated* in geometrical language; for instance, that the stars describe circles about the Pole-star, or that the different positions of the sun among the fixed stars in the course of the year form a circle. For astronomical calculations it was found necessary to determine the arc of a circle by means of its chord. The notion is as old as Hipparchus, a work of whom is referred to as consisting of twelve books on the chords of circular arcs. We have (A.D. 125) Ptolemy's 'Almagest,' the first book of which contains a table of arcs and chords, with the method of construction; and among other theorems on the subject, he gives there the theorem, afterwards inserted in Euclid (book vi. prop. D), relating to the rectangle contained by the diagonals of a quadrilateral inscribed in a circle. The Arabians made the improvement of using, in place of the chord of an arc, the sine, or half-chord of double the arc, and so brought the theory into the form in which it is used in modern trigonometry. The before-mentioned theorem of Ptolemy, — or, rather, a particular case of it, — translated into the notation of sines, gives the expression for the sine of the sum of two arcs in terms of the sines and cosines of the component arcs, and it is thus the fundamental theorem on the subject. We have in the fifteenth and sixteenth centuries a series of mathematicians, who, with wonderful enthusiasm and perseverance, calculated tables of the trigonometrical or circular functions, — Purbach, Müller or Regiomontanus,

[1] Address of Professor CAYLEY before the British association. Concluded from No. 35.

Copernicus, Reinhold, Maurolycus, Vieta, and many others. The tabulations of the functions tangent and secant are due to Reinhold and Maurolycus respectively.

Logarithms were invented, not exclusively with reference to the calculation of trigonometrical tables, but in order to facilitate numerical calculations generally. The invention is due to John Napier of Merchiston, who died in 1618, at sixty-seven years of age. The notion was based upon refined mathematical reasoning on the comparison of the spaces described by two points; the one moving with a uniform velocity, the other with a velocity varying according to a given law. It is to be observed that Napier's logarithms were nearly, but not exactly, those which are now called, sometimes Napierian, but more usually hyperbolic logarithms, those to the base e; and that the change to the base 10 (the great step by which the invention was perfected for the object in view) was indicated by Napier, but actually made by Henry Briggs, afterwards Savilian professor at Oxford (d. 1630). But it is the hyperbolic logarithm which is mathematically important. The direct function e^x, or exp. x, which has for its inverse the hyperbolic logarithm, presented itself, but not in a prominent way. Tables were calculated of the logarithms of numbers, and of those of the trigonometrical functions.

The circular function and the logarithm were thus invented each for a practical purpose, separately, and without any proper connection with each other. The functions are connected through the theory of imaginaries, and form together a group of the utmost importance throughout mathematics: but this is mathematical theory; the obligation of mathematics is for the discovery of the functions.

Forms of spirals presented themselves in Greek architecture, and the curves were considered mathematically by Archimedes. The Greek geometers invented some other curves more or less interesting, but recondite enough in their origin. A curve which might have presented itself to anybody, that described by a point in the circumference of a rolling carriage-wheel, was first noticed by Mersenne in 1615, and is the curve afterwards considered by Roberval, Pascal, and others, under the name of the roulette, otherwise the cycloid. Pascal (1623–62) wrote, at the age of seventeen, his 'Essais pour les coniques' in seven short pages, full of new views on these curves, and in which he gives, in a paragraph of eight lines, his theory of the inscribed hexagon.

Kepler (1571–1630), by his empirical determination of the laws of planetary motion, brought into connection with astronomy one of the forms of conic, the ellipse, and established a foundation for the theory of gravitation. Contemporary with him for most of his life, we have Galileo (1564–1642), the founder of the science of dynamics; and closely following upon Galileo, we have Isaac Newton (1643–1727). The 'Philosophiae naturalis principia mathematica,' known as the 'Principia,' was first published in 1687.

The physical, statical, or dynamical questions which presented themselves before the publication of the 'Principia' were of no particular mathematical difficulty; but it is quite otherwise with the crowd of interesting questions arising out of the theory of gravitation, and which, in becoming the subject of mathematical investigation, have contributed very much to the advance of mathematics. We have the problem of two bodies, or, what is the same thing, that of the motion of a particle about a fixed centre of force, for any law of force; we have also the problem (mathematically very interesting) of the motion of a body attracted to two or more fixed centres of force; then, next preceding that of the actual solar system, the problem of three bodies. This has ever been and is far beyond the power of mathematics; and it is in the lunar and planetary theories replaced by what is mathematically a different problem, — that of the motion of a body under the action of a principal central force and a disturbing force, — or, in one mode of treatment, by the problem of disturbed elliptic motion. I would remark that we have here an instance in which an astronomical fact, the observed slow variation of the orbit of a planet, has directly suggested a mathematical method, applied to other dynamical problems, and which is the basis of very extensive modern investigations in regard to systems of differential equations. Again: immediately arising out of the theory of gravitation, we have the problem of finding the attraction of a solid body of any given form upon a particle, solved by Newton in the case of a homogeneous sphere, but which is far more difficult in the next succeeding cases of the spheroid of revolution (very ably treated by Maclaurin), and of the ellipsoid of three unequal axes. There is, perhaps, no problem of mathematics which has been treated by so great a variety of methods, or has given rise to so much interesting investigation, as this last problem of the attraction of an ellipsoid upon an interior or exterior point. It was a dynamical problem, that of vibrating strings, by which Lagrange was led to the theory of the representation of a function as the sum of a series of multiple sines and cosines; and connected with this we have the expansions in terms of Legendre's functions P_n, suggested to him by the question, just referred to, of the attraction of an ellipsoid. The subsequent investigations of Laplace, on the attractions of bodies differing slightly from the sphere, led to the functions of two variables called Laplace's functions. I have been speaking of ellipsoids; but the general theory is that of attractions, which has become a very wide branch of modern mathematics. Associated with it, we have in particular the names of Gauss, Lejeune-Dirichlet, and Green; and I must not omit to mention that the theory is now one relating to n-dimensional space. Another great problem of celestial mechanics, that of the motion of the earth about its centre of gravity (in the most simple case, that of a body not acted upon by any forces), is a very interesting one in the mathematical point of view.

I may mention a few other instances where a practical or physical question has connected itself with the development of mathematical theory. I have spoken of two map projections, — the stereographic, dating from Ptolemy; and Mercator's projection, in-

vented by Edward Wright about the year 1600. Each of these, as a particular case of the orthomorphic projection, belongs to the theory of the geometrical representation of an imaginary variable. I have spoken also of perspective, and (in an omitted paragraph) of the representation of solid figures employed in Monge's descriptive geometry. Monge, it is well known, is the author of the geometrical theory of the curvature of surfaces, and of curves of curvature. He was led to this theory by a problem of earthwork, — from a given area, covered with earth of uniform thickness, to carry the earth and distribute it over an equal given area with the least amount of cartage. For the solution of the corresponding problem in solid geometry, he had to consider the intersecting normals of a surface, and so arrived at the curves of curvature (see his 'Mémoire sur les déblais et les remblais,' *Mém. de l'acad.*, 1781). The normals of a surface are, again, a particular case of a doubly infinite system of lines, and are so connected with the modern theories of congruences and complexes.

The undulatory theory of light led to Fresnel's wave-surface, — a surface of the fourth order, by far the most interesting one which had then presented itself. A geometrical property of this surface, that of having tangent planes, each touching it along a plane curve (in fact, a circle), gave to Sir W. R. Hamilton the theory of conical refraction. The wave-surface is now regarded in geometry as a particular case of Kummer's quartic surface, with sixteen conical points and sixteen singular tangent planes.

My imperfect acquaintance, as well with the mathematics as the physics, prevents me from speaking of the benefits which the theory of partial differential equations has received from the hydrodynamical theory of vortex motion, and from the great physical theories of electricity, magnetism, and energy.

It is difficult to give an idea of the vast extent of modern mathematics. This word 'extent' is not the right one: I mean extent crowded with beautiful detail, — not an extent of mere uniformity, such as an objectless plain, but of a tract of beautiful country seen at first in the distance, but which will bear to be rambled through, and studied in every detail of hillside and valley, stream, rock, wood, and flower. But as for any thing else, so for a mathematical theory, — beauty can be perceived, but not explained. As for mere extent, I might illustrate this by speaking of the dates at which some of the great extensions have been made in several branches of mathematical science.

And, in fact, in the address as written, I speak at considerable length of the extensions in geometry since the time of Descartes, and in other specified subjects since the commencement of the century. These subjects are the general theory of the function of an imaginary variable; the leading known functions, viz., the elliptic and single theta-functions and the Abelian and multiple theta-functions; the theory of equations and the theory of numbers. I refer also to some theories outside of ordinary mathematics, — the multiple algebra, or linear associative algebra, of the late Benjamin Peirce; the theory of Argand, Warren, and Peacock, in regard to imaginaries in plane geometry; Sir W. R. Hamilton's quaternions; Clifford's biquaternions; the theories developed in Grassmann's 'Ausdehnungslehre,' with recent extensions thereof to non-Euclidian space by Mr. Homersham Cox; also Boole's 'Mathematical logic,' and a work connected with logic, but primarily mathematical and of the highest importance, Shubert's 'Abzählende geometrie' (1878). I remark that all this in regard to theories outside of ordinary mathematics is still on the text of the vast extent of modern mathematics.

In conclusion, I would say that mathematics have steadily advanced from the time of the Greek geometers. Nothing is lost or wasted. The achievements of Euclid, Archimedes, and Apollonius, are as admirable now as they were in their own days. Descartes' method of co-ordinates is a possession forever. But mathematics has never been cultivated more zealously and diligently, or with greater success, than in this century, — in the last half of it, or at the present time. The advances made have been enormous. The actual field is boundless, the future full of hope. In regard to pure mathematics we may most confidently say, —

> "Yet I doubt not through the ages one increasing purpose runs,
> And the thoughts of men are widened with the process of the suns."

THE ENDOWMENT OF BIOLOGICAL RESEARCH.[1]

It has become the custom for the presidents of the various sections of this association to open the proceedings of the departments with the chairmanship of which they are charged by formal addresses. In reflecting on the topics which it might be desirable for me to bring under your notice, as your president, on the present occasion, it has occurred to me that I might use this opportunity most fitly by departing somewhat from the prevailing custom of reviewing the progress of science in some special direction during the past year, and that, instead of placing before you a summary of the results recently obtained by the investigations of biologists in this or that line of inquiry, I might ask your attention, and that of the external public (who are wont to give some kindly consideration to the opinions expressed on these occasions) to a matter which is even more directly connected with the avowed object of our association; namely, 'the advancement of science.' I propose to place before you a few observations upon the provision which exists in this country for the advancement of that branch of science to which section D is dedicated; namely, biology.

I am aware that it is usual for those who speak of men of science and their pursuits to ignore altogether such sordid topics as the one which I have chosen to bring forward. A certain pride, on the one hand, and a willing acquiescence, on the other hand, usually prevent those who are professionally concerned with

[1] An address to the biological section of the British association. By Prof. E. Ray Lankester, M.A., F.R.S., F.L.S., president of the section. From advance copy kindly furnished by the editor of *Nature*.

scientific pursuits from exposing to the public the pecuniary destitution, and the consequent crippling and languor, of scientific research in this country. Those Englishmen who take an interest in the progress of science are apt to suppose, that, in some way which they have never clearly understood, the pursuit of scientific truth is not only its own reward, but also a sufficient source of food, drink, and clothing. Whilst they are interested and amused by the remarkable discoveries of scientific men, they are astonished whenever a proposal is mentioned to assign salaries to a few such persons, sufficient to enable them to live decently whilst devoting their time and strength to investigation. The public are becoming more and more anxious to have the opinion or report of scientific men upon matters of commercial importance, or in relation to the public health; and yet, in ninty-nine cases out of a hundred, they expect to have that opinion for the asking, although accustomed to pay other professional men handsomely for similar service. There is, it appears, in the public mind, a vague belief that men who occupy their time with the endeavor to add to knowledge in this or that branch of science are mysteriously supported by the state exchequer, and are thus fair game for attacking with all sorts of demands for gratuitous service; or, on the other hand, the notion at work appears sometimes to be, that the making of new knowledge — in fact, scientific discovery — is an agreeable pastime, in which some ingenious gentlemen, whose business in other directions takes up their best hours, find relaxation after dinner or on the spare hours of Sunday. Such mistaken views ought to be dispelled with all possible celerity and determination. It is in part owing to the fact that the real state of the case is not widely and persistently made known to the public, that no attempt is made in this country to raise scientific research, and especially biological research, from the condition of destitution and neglect under which it suffers, — a condition which is far below that of these same interests in France and Germany, and even in Holland, Belgium, Italy, and Russia, and is discreditable to England in proportion as she is richer than other states.

It appears to me, that, in placing this matter before you, I may remove myself from any suggestion of self-interest by at once stating that the great defect to which I shall draw your attention is, *not* that the few existing public positions which are open in this country to men who intend to devote their chief energies to biological research are endowed with insufficient salaries, but that there is not any thing like *a sufficiently large number* of those posts, and that there is in that respect, from a national point of view, a pecuniary starvation of biology, a withholding of money, which (to use another metaphor) is no less the sinews of the war of science against ignorance than of other less glorious campaigns. Surely, men engaged in the scientific profession may advocate the claim of science to maintenance and needful pecuniary provision. It seems to me that we should, if necessary, swallow, rather than be controlled by, that pride which tempts us to paint the scientific career as one far above and independent of pecuniary considerations; whereas all the while we know that knowledge is languishing, that able men are drawn off from scientific research into other careers, that important discoveries are approached and their final grasp relinquished, that great men depart, and leave no disciples or successors, simply for want of that which is largely given in other countries, — of that which is most abundant in this country, and is so lavishly expended on armies and navies, on the development of commercial resources, on a hundred injurious or meaningless charities, — viz., money.

I have no doubt that I have the sympathy of all my hearers in wishing for more extensive provision in this country for the prosecution of scientific research, and especially of biological research. I need hardly remind this audience of the almost romantic history of some of the great discoveries which have been made in reference to the nature and history of living things during the past century. The microscope, which was a drawing-room toy a hundred years ago, has, in the hands of devoted and gifted students of nature, been the means of giving us knowledge which, on the one hand, has saved thousands of surgical patients from terrible pain and death, and, on the other hand, has laid the foundation of that new philosophy with which the name of Darwin will forever be associated. When Ehrenberg, and, later, Dujardin, described and figured the various forms of Monas, Vibrio, Spirillum, and Bacterium, which their microscopes revealed to them, no one could predict that fifty years later these organisms would be recognized as the cause of that dangerous suppuration of wounds which so often defeated the beneficent efforts of the surgeon, and made an operation in a hospital-ward as dangerous to the patient as residence in a plague-stricken city. Yet this is the result which the assiduous studies of the biologists, provided with laboratories and maintenance by continental states, have in due time brought to light. Theodore Schwann, professor at Liége, first showed that these bacteria are the cause of the putrefaction of organic substances; and subsequently, the French chemist Pasteur, professor in the École normale of Paris, confirmed and extended Schwann's discovery, so as to establish the belief that all putrefactive changes are due to such minute organisms, and that, if these organisms can be kept at bay, no putrefaction can occur in any given substance.

It was reserved for our countryman, Joseph Lister, to apply this result to the treatment of wounds, and, by his famous antiseptic method, to destroy by means of special poisons the putrefactive organisms which necessarily find their way into the neighborhood of a wound, or of the surgeon's knife and dressings, and to ward off by similar means the access of such organisms to the wounded surface. The amount of death, not to speak of the suffering short of death, which the knowledge of bacteria gained by the microscope has thus averted, is incalculable.

Yet, further, the discoveries of Ehrenberg, Schwann, and Pasteur, are bearing fruit of a similar kind in

other directions. It seems in the highest degree probable that the terrible scourge known as tubercular consumption, or phthisis, is due to a parasitic bacterium (Bacillus) discovered two years since by Koch of Berlin as the immediate result of investigations which he was commissioned to carry on at the public expense, in the specially erected laboratory of public health, by the German imperial government. The diseases known as erysipelas and glanders (or farcy) have similarly, within the past few months in German state-supported laboratories, been shown to be due to the attacks of special kinds of bacteria. At present this knowledge has not led to a successful method of combating those diseases, but we can hardly doubt that it will ultimately do so. We are warranted in this belief by the fact that the disease known as 'splenic fever' in cattle, and 'malignant pustule,' or anthrax, in man, has likewise been shown to be due to the action of a special kind of bacterium, and that this knowledge has, in the hands of MM. Toussaint and Pasteur, led to a treatment, in relation to this disease, similar to that of vaccination in relation to small-pox. By cultivation a modified growth of the anthrax parasite is obtained, which is then used in order to inoculate cattle and sheep with a mild form of the disease, such inoculation having the result of rendering the cattle and sheep free from the attacks of the severe form of disease, just as vaccination or innoculation with cow-pox protects man from the attack of the deadly small-pox. One other case I may call to mind, in which knowledge of the presence of bacteria as the cause of disease has led to successful curative treatment. A not uncommon affliction is inflammation of the bladder, accompanied by ammoniacal decomposition of the urine. Microscopical investigation has shown that this ammoniacal decomposition is entirely due to the activity of a bacterium. Fortunately, this bacterium is at once killed by weak solutions of quinine, which can be injected into the bladder without causing any injury or irritation. This example appears to have great importance; because it is the fact that many kinds of bacteria are not killed by solutions of quinine, but require other and much more irritant poisons to destroy their life, which could not be injected into the bladder without causing disastrous effects. Since some bacteria are killed by one poison, and some by another, it becomes a matter of the keenest interest to find out all such poisons; and possibly among them may be some which can be applied so as to kill the bacteria which produce phthisis, erysipelas, glanders, anthrax, and other scourges of humanity, whilst not acting injuriously upon the body of the victim in which these infinitesimal parasites are doing their deadly work. In such ways as this, biology has turned the toy 'magnifying-glass' of the last century into a saver of life and health.

No less has the same agency revolutionized the thoughts of men in every branch of philosophy and speculation. The knowledge of the growth of the chick from the egg, and of other organisms from similarly constituted beginnings, has been slowly and continuously gained by prodigious labor, extending over generation after generation of students who have occupied the laboratories, and lived on the stipends, provided by the governments of European states, — not English, but chiefly German. It is this history of the development of the individual animal and plant from a simple homogeneous beginning to a complex heterogeneous adult, which has furnished the starting-point for the wide-reaching doctrine of evolution. It is this knowledge, coupled with the knowledge of the myriad details of structure of all kinds of animals and plants, which the faithful occupants of laboratories, and the guardians of biological collections, have, in the past hundred years, laboriously searched out, and recorded. It is this which enabled Darwin to propound, to test, and to firmly establish his theory of the origin of species by natural selection, and finally to bring the origin, development, and progress of man also into the area of physical science. I have said enough, in referring only to two very diverse examples of the far-reaching consequences flowing from the discoveries of single-minded investigators in biological science, to remind my hearers that in the domain of biology, as in other sciences, the results attained by those who have labored simply to extend our knowledge of the structure and properties of living things, in the faith that every increase of knowledge will ultimately bring its blessing to humanity, have, in fact, led with astonishing rapidity to conclusions affecting most profoundly both the bodily and the mental welfare of the community.

We who know the beneficent results which must flow more and more from the labors of those who are able to create new knowledge of living things, or, in other words, are able to aid in the growth of biological science, must feel something more than regret, — even indignation, — that England should do so small a proportion of the laborious investigation which is necessary, and is being carried on for our profit by other nationalities. It must not be supposed, because we have had our Harvey and our Darwin, our Hunter and our Lister, that therefore we have done, and are doing, all that is needful in the increase of biological science. The position of this country in relation to the progress of science is not to be decided by the citation of great names.

We require to look more fully into the matter than this. The question is, not whether England has produced some great discoverers, or as many as any other nationality, but whether we might not, with advantage to our own community and that of the civilized world generally, do far more in the field of scientific investigation than we do.

It may be laid down as a general proposition, to which I know of no important exception, that scientific discovery has only been made by one of two classes of men; namely, (1) those whose time could be devoted to it in virtue of their possessing inherited fortunes; (2) those whose time could be devoted to it in virtue of their possessing a stipend or endowment especially assigned to them for that purpose.

Now, it is a very remarkable fact that in England, far more than in any other country, the possessors of

private fortunes have devoted themselves to scientific investigation. Not only have we, in all parts of the country, numerous *dilettanti*,[1] who, especially in various branches of biology, do valuable work in continually adding to knowledge, quietly pursuing their favorite study without seeking to reach to any great eminence, but it is the fact that many of the greatest names of English discoverers in science are those of men who held no professional position designed to maintain an investigator, but owed their opportunity simply to the fact that they enjoyed a more or less ample income by inheritance. Thus, Harvey possessed a private fortune, Darwin also, and Lyell. Such, also, is true of some of the English naturalists, who more recently have most successfully devoted their energies to research. Those who wish to defend the present neglect of the government and of public institutions to provide means for the carrying on of scientific research in this country are accustomed to declare as a justification for this neglect, that we do very well without such provision, inasmuch as the cultivation of science here flourishes in the hands of those who are in a position of pecuniary independence. The reply to this is obvious. If those few of our countrymen who by accident are placed in an independent position show such ability in the prosecution of scientific research, how much more would be effected in the same direction, were the machinery provided to enable those also who are *not* accidentally favored by fortune to enter upon the same kind of work! The number of wealthy men who have distinguished themselves in scientific research in England is simply evidence that there is a natural ability and liking for such work in the English character, and is a distinct encouragement to those who have it in their power to do so, to offer the opportunity of devoting themselves to research to a larger number of the members of the community. It is impossible to doubt that there are hundreds of men amongst us who have as great capacity for scientific discovery as those whom fortune has favored with leisure and opportunity. It cannot be doubted, that, were the means provided to enable even a proportion of such men to give themselves up to scientific investigation, great discoveries, of no less importance to the world than those relative to the causes of disease, and the development of living things from the egg, — which I have cited, — would be made as a direct consequence of their activity; whereas now we must wait until, in due course of time, these discoveries shall be made for us in the laboratories of Germany, France, or Russia.

It should further be pointed out, that it is altogether a mistake to suppose that the existence amongst us of a few very eminent men is any evidence that we are contributing largely to the hard work of careful study and observation, which really forms the material upon which the conclusions of eminent discoverers are based. You will find in every department of biological knowledge, that the hard work of investigation is being carried on by the well-trained army of German observers. Whether you ask the zoölogist, the botanist, the physiologist, or the anthropologist, you will get the same answer: it is to German sources that he looks for new information; it is in German workshops that discoveries, each small in itself, but gradually leading up to great conclusions, are daily being made. To a very large extent, the business of those who are occupied with teaching or applying biological science in this country consists in making known what has been done in German laboratories. Our English students flock to Germany to learn the methods of scientific research; and to such a state of weakness is English science reduced, for want of proper nurture and support, that, even on some of the rare occasions when a capable investigator of biological problems has been required for the public service, it has been necessary to obtain the assistance of a foreigner trained in the laboratories of Germany.

Let me now briefly explain what are the arrangements, in number and in kind, which exist in other countries, for the purpose of promoting the advancement of biological science, which are wanting in this country.

In the German empire, with a population of 45,000,000, there are twenty-one universities. These universities are very different from any thing which goes by the name in this country. Amongst its other arrangements, devoted to the study and teaching of all branches of learning and science, each university has five institutes, or establishments, devoted to the prosecution of researches in biological science. These are respectively the physiological, the zoölogical, the anatomical, the pathological, and the botanical. In one of these universities of average size, each of the institutes named consists of a spacious building containing many rooms fitted as workshops, provided with instruments, a museum, and, in the last instance, with an experimental garden. All this is provided and maintained by the state. At the head of each institute is the university professor respectively of physiology, of zoölogy, of anatomy, of pathology, or of botany. He is paid a stipend by the state, which, in the smallest university, is as low as £120, but may be in others as much as £700, and averages, say, £400 a year. Considering the relative expenditure of the professional classes in the two countries, this average may be taken as equal to £800 a year in England.[1] Besides the professor, each institute has attached to it, with salaries paid by the state, two qualified assistants, who, in course of time, will succeed to independent positions. A liberal allowance is also made to each institute, by the state, for the purchase of instruments, material for study, and for the pay of servants; so that the total expenditure on professor, assistants, laboratory service, and

[1] I use this word in its best and truest sense, and would refer those who have been accustomed to associate with it some implication of contempt to the wise and appreciative remarks of Goethe on 'dilettanti.'

[1] From the fact that the salaries of judges, civil servants, military and naval officers, parsons, and schoolmasters, as also the fees of physicians and lawyers, are in Germany even less than half what is paid to their representatives in England, I think that we are justified in making this estimate.

maintenance, averages £800 a year for each institute, reaching as much as £2,000 or £3,000 a year in the larger universities. It is the business of the professor, in conjunction with his assistants and the advanced students, who are admitted to work in the laboratories free of charge, to carry on investigations, *to create new knowledge* in the several domains of physiology, zoölogy, anatomy, pathology, and botany. It is for this that the professor receives his stipend, and it is on his success in this field of labor that his promotion to a more important or better paid post in another university depends. In addition to and irrespective of this part of his duties, each professor is charged with the delivery of courses of lectures, and of elementary instruction to the general students of the university; and for this he is allowed to charge a certain fee to each student, which he receives himself. The total of such fees may, in the case of a largely attended university and a popular subject, form a very important addition to the professorial income; but it is distinctly to be understood that such payment by fees is only an *addition* to the professor's income, quite independent of his stipend, and of his regular occupation in the laboratory: it is paid from a separate source, and for a separate object. There are thus in the German empire more than 100 such institutes devoted to the prosecution of biological discovery, carried on at an annual cost to the state of about £80,000, equal to about £160,000 in England, providing posts of graduated value for 300 investigators, some of small value, sufficient to carry the young student through the earlier portion of his career, whilst he is being trained and acting as the assistant of more experienced men; others forming the sufficient but not too valuable prizes which are the rewards of continuous and successful labor.

In addition to these university institutes, there are in Germany such special laboratories of research, with duly salaried staff of investigators, as the Imperial sanitary institute of Berlin, and the large museums of Berlin, Bremen, and other large towns, corresponding to our own British museum of natural history.

Moreover, we must be careful to note, in making any comparison with the arrangements existing in England, that there are, in addition to the universities in Germany, a number of other educational institutions, at least equal in number, which are known as polytechnic schools, technical colleges, and agricultural colleges. These furnish posts of emolument to a limited number of biological students, who give courses of instruction to their pupils; but they have not the same arrangements for research as the universities, and are closely similar to those colleges which have been founded of late years in the provincial towns of England, such as Bristol, Nottingham, and Leeds. The latter are sometimes quoted by sanguine persons, who are satisfied with the neglected condition of scientific training and research in this country, as really sufficient and adequate representatives of the German universities. As a matter of fact, the excellent English colleges in question do not present any thing at all comparable to the arrangements of a German university, and are, in respect of the amount of money which is expended upon them, the number of their teaching-staff, and the efficiency of their laboratories, inferior not merely to the smallest German university, but inferior to many of the technical schools of that country.

Passing from Germany, I would now ask your attention for a moment to an institution which is supported by the French government, and which — quite irrespective of the French university system, which is not, on the whole, superior to our own — constitutes one of the most effective arrangements, in any European state, for the production of new knowledge. The institution to which I allude is the Collége de France in Paris, — co-existing there with the Sorbonne, the École de médecine, the École normale, the Jardin des plantes, and other state-supported institutions, — in which opportunity is provided for those Frenchmen who have the requisite talent to pursue scientific discovery in the department of biology, and in other branches of science. I particularly mention the Collége de France, because it appears to me that the foundation of such a college in London would be one of the simplest and most direct steps that could be taken towards filling, in some degree, the void from which English science suffers. The Collége de France is divided into a literary and a scientific faculty. Each faculty consists of some twenty professors. Each professor in the scientific faculty is provided with a laboratory and assistants (as many as four assistants in some cases), and with a considerable allowance for the expenses of the instruments and materials required in research. The personal stipend of each professor is £400, which has been increased by an additional £100 a year in some cases from the government department charged with the promotioh of higher studies. The professors in this institution, as in the German universities, when a vacancy occurs, have the right of nominating their future colleague, their recommendation being accepted by the government. The professors are not expected to give any elementary instruction, but are directed to carry on original investigations, in prosecuting which, they may associate with themselves pupils who are sufficiently advanced to join in such work; and it is further the duty of each professor to give a course of forty lectures in each year, upon the results of the researches in which he is engaged. There are at present, among the professors of the Collége de France, four of the most distinguished among contemporary students of biological science, — Professor Brown-Sequard, Professor Marey, Professor Balbiani, and Professor Ranvier. Every one who is acquainted with the progress of discovery in physiology, minute anatomy, and embryology, will admit that the opportunities afforded to these men have not been wasted. They have, as the result of the position in which they have been placed, produced abundant and most valuable work, and have, in addition, trained younger men to carry on the same line of activity. It was here, too, in the Collége de France, that the great genius of Claude Bernard found the necessary conditions for its development.

Let us now see how many and what kind of institutions there are in England devised so as to promote the making of new knowledge in biological science. Most persons are apt to be deceived in this matter by the fact that the terms 'university,' 'professorship,' and 'college' are used very freely in England in reference to institutions which have no pecuniary resources whatever, and which, instead of corresponding to the German arrangements which go by these names, are empty titles, neither backed by adequate subsidy of the state nor by endowment from private sources.

In England, with its 25,000,000 inhabitants, there are only four universities which possess endowments and professoriates: viz., Oxford, Cambridge, Durham, and the Victoria (Owens college). Besides these, which are variously and specially organized each in its own way, there are the London colleges (University and King's), the Normal school of science at South Kensington, and various provincial colleges, which are, to a small and varying extent, in possession of funds which could be or are used to promote scientific research. Amongst all these variously arranged institutions, there is an extraordinarily small amount of provision for biological research. In London there is one professorship only, that at the Normal school of science, which is maintained by a stipend paid by the state, and has a laboratory and salaried assistants similarly maintained, in connection with it. The only other posts in London which are provided with stipends intended to enable their holders to pursue researches in the domain of biological science, are the two chairs of physiology and of zoölogy at University college, which, through the munificence of a private individual,[1] have been endowed to the extent of £300 a year each. To these should be added, in our calculation, certain posts in connection with the British museum of natural history and the Royal gardens at Kew, maintained by the state; though it must be remembered that a large part of the expenditure in those institutions is necessarily taken up in the preservation of great national collections, and is not applicable to the subvention of investigators. We may, however, reckon about six posts, great and small, in the British museum, and four at Kew, as coming into the category which we have in view. In London, then, we may reckon approximately some fourteen or fifteen subsidized posts for biological research. In Oxford there fall under this category the professorship of anatomy and his assistant, that of physiology, that of zoölogy, that of botany. The Oxford professorships are well supported by endowment, averaging £700 or £800 a year; but they are inadequately provided with assistants, as compared with corresponding German positions. Whilst Oxford has thus five posts, Cambridge has at present the same number, though the stipends are of less average value. In regard to Durham, it does not appear that the biological professorships (which have their seat in the Newcastle college of science) are supported by stipends derived from endowment: they fall under another category, to which allusion will be made below, of purely teaching positions, supported by the fees paid for such teaching by pupils. The Victoria university (Owens college, Manchester) supports its professors of physiology, anatomy, zoölogy, botany, and pathology, by means partly of endowment, partly of pupils' fees. By the provision of adequate laboratories, and of salaries for assistants to each professor, and of student-fellowships, Owens college gives direct support to orginal investigation. We may reckon five major and eight minor posts as dedicated to biological research in this college. Altogether, then, we have fifteen positions in London and twenty-three in the provinces (taking assistantships and professorships and curatorships together), — a total of thirty-eight in all England, with its 25,000,000 inhabitants, as against the three hundred in Germany, with 45,000,000 inhabitants. In proportion to its population (leaving aside the consideration of its greater wealth), England has only about one-fourth of the provision for the advancement of biological research which exists in Germany.

It would not be fair to reckon in this comparison the various biological professorships in small colleges recently created, and paid to a small extent by stipends derived from endowments in the provincial towns of England: for the holders of these chairs are called upon to teach a variety of subjects; for instance, zoölogy, botany, and geology combined. And not only is the devotion of the energies of their teaching-staff to scientific discovery not contemplated in the arrangement of these institutions, but, as a matter of fact, the large demands made on the professors in the way of teaching must deprive them of the time necessary for any serious investigation. Such posts, in the fact that neither time, assistants, nor proper laboratories are provided to enable their holders to engage in scientific research, are schoolmasterships rather than professorships, as the word is used in German universities.

One result of the exceedingly small provision of positions in England, similar to those furnished by the German university system, and of the irregular, uncertain character of many of those which do exist, is, that there is an insufficient supply of young men willing to enter upon the career of zoölogist, botanist, physiologist, or pathologist, as a profession. The number of posts is too small to create a profession, i.e., an avenue of success; and consequently, whereas in Germany there is always a large body of new men ready to fill up the vacancies as they occur in the professorial organization, in England it very naturally does not appear to our university students as a reasonable thing to enter upon research as a profession, when the chances of employment are so few, and far between.

Before stating, as I propose to do, what appears to me a reasonable and proper method of removing, to some extent, the defect in our national life due to the want of provision for scientific research, I will endeavor to meet some of the objections which are

[1] Mr. Jodrell.

usually raised to such views as those which I am advocating. The endowment of research by the state, or from public funds of any kind, is opposed on various grounds. One is, that such action on the part of the government is well enough in continental states, but is contrary to the spirit of English statecraft, which leaves scientific as well as other *enterprise* to the individual initiative of the people. This objection is based on error, both as to fact and theory. It is well enough to leave to individual effort the conduct of such enterprises as are remunerative to the parties who conduct them; but it is a mistake to speak of scientific research as an 'enterprise' at all. The mistake arises from the extraordinary pertinacity with which so-called 'invention' is confounded with the discovery of scientific truth. New knowledge in biological or other branches of science cannot be sold: it has no marketable value. Koch could not have sold the discovery of the bacterium of phthisis for as much as sixpence, had he wished to do so. Accordingly, we find that there is not, and never has been, any tendency among the citizens of this country to provide for themselves institutions for the manufacture of an article of so little pecuniary value to the individual who turns it out as is new knowledge. On the other hand, as a matter of fact, the providing of means for the manufacture of that article is not only not foreign to English statecraft, but is largely, though not largely enough, undertaken by the English state. The Royal observatories, the British museum, the Royal gardens at Kew, the Geological survey, the government grant of £4,000 a year to the Royal society, the £300 or £400 a year (not a large sum) expended through the medical officer of the privy council upon the experimental investigation of disease, are ample evidence that such providing of means for creating new knowledge forms part of the natural and recognized responsibilities of the British government. Such a responsibility clearly is recognized in this country, and does fall, according to the present arrangement of things, upon the central government. What we have to regret is, that those who temporarily hold the reins of government fail to perceive the lamentable inadequacy of the mode in which this responsibility is met.

A second objection which is made to the endowment of research by public funds, or by other means, such as voluntary contributions, is this: it is stated that men engaged in scientific research ought to *teach*, and thus gain their livelihood. It is argued, in fact, that there is no need whatever to provide stipends or laboratories for researchers, since they have only to stand up and teach in order to make income sufficient to keep them and their families, and to provide themselves with laboratories. This is a very plausible statement, because it is the fact that some investigators have also been excellent lecturers, and have been able to make an income by teaching, whilst carrying on a limited amount of scientific investigation. But neither by teaching in the form of popular lectures, nor by teaching university or professional students who desire, as a result, to pass some examination-test, is it possible, where there is a fair field and no favor, for a man to gain a reasonable income, and at the same time to leave himself time and energy to carry on original investigations in science.

In some universities, such as those of Scotland, the privilege of conferring degrees of pecuniary value to their possessors becomes a source of income to the professors of the university. They are, in fact, able to make considerable incomes, independently of endowment, by compelling the candidates for degrees to pay a fee to each professor in the faculty for the right of attending his lectures, and of presentation to the degree: consequently teaching here appears to be producing an income which may support a researcher. In reality, it is the acquisition of the university degree, and not necessarily the teaching, for which the pupil pays his fee. Where the teacher is unprotected by any compulsory regulations (such as that which requires attendance on his lectures, and fee-payment on the part of the pupils), it is *impossible* for him to obtain such an income, by teaching for one hour a day, as will enable him to devote the rest of the day to unremunerative study and investigation, for the following reason. Other teachers, equally satisfactory as teachers, will enter into competition with him, without having the same intention of teaching for one hour only, and of carrying on researches for the rest of the day. They will contemplate teaching for six hours a day, and they will accordingly offer to those who require to be taught, either six hours' teaching for the same fee which the researcher charges for one, or one hour for a sixth part of that fee: consequently the unprotected researcher will find his lecture-room deserted. Pupils will naturally go to the equally good teacher who gives more teaching for the same fee, or the same teaching for a less cost. And no one can say that this is not as it should be. The university pupil requires a certain course of instruction, which he ought to be able to buy at the cheapest rate. It does not seem to be doing justice to the pupil, to compel him to form one of a class consisting of some hundreds of hearers, where he can obtain but little personal supervision or attention from the teacher, whereas, if he had the free disposal of his fee, he might obtain six times the amount of attention from another teacher. This arrangement does not seem to be justifiable, even for the purpose of providing the university professor with an income, and leisure to pursue scientific research. The student's fee should pay for a given amount of teaching at the market value; and he has just cause of complaint, if, by compulsory enactments, he is taxed to provide the country with scientific investigation.

Teaching must, in all fairness, ultimately be paid for as teaching; and scientific research must be provided for out of other funds than those extracted from the pockets of needy students, who have a reasonable right to demand, in return for their fees, a full modicum of instruction and direction in study.

In the German universities, the professor receives a stipend which provides for him as an investigator. He also gives lectures, for which he charges a fee;

but no student is compelled to attend those lectures as a condition of obtaining his degree. Accordingly, independent teachers can and do compete with the professor in providing for the students' requirements in the matter of instruction. As a consequence, the fees charged for teaching are exceedingly small, and the student can feel assured that he is obtaining his money's worth for his money. He is not compelled to pay any fee to any teacher as a condition of his promotion to the university degree. In a German university, if the professor in a given subject is incompetent, or the class overcrowded, the student can take his fee to a private teacher, and get better teaching. All that is required of the candidate, as a condition of his promotion to the doctor's degree, is that he shall satisfy the examination-tests imposed by the faculty, and produce an original thesis.

Unless there be some such compelling influence as that obtaining in the Scotch universities, enabling the would-be researcher to gather to him pupils and fees without fear of competition, it seems impossible that he should gain an income by teaching, whilst reserving to himself time and energy for the pursuit of scientific inquiry. It is thus seen that the necessity of endowment, in some form or another, to make provision for scientific research, is a reality, in spite of the suggestion that teaching affords a means whereby the researcher may readily provide for himself. The simple fact is, that a teacher can only make a sufficient income by teaching, on the condition that he devotes his whole time and energy to that occupation.

Whilst I feel called upon to emphatically distinguish the two functions, — viz., that of *creating new knowledge*, and that of *distributing existing knowledge*, — and to maintain that it is only by arbitrary and undesirable arrangements not likely to be tolerated, or, at any rate, extended, at the present day, that the latter can be made to serve as the support of the former, I must be careful to point out that I agree most cordially with those who hold that it is an excellent thing for a man who is engaged in the one to give a certain amount of time to the other. It is a matter of experience, that the best teachers of a subject are, *ceteris paribus*, those who are actually engaged in the advancement of that subject, and who have shown such a thorough understanding of that subject as is necessary for making new knowledge in connection with it. It is also, in most cases, a good thing for the man engaged in research to have a certain small amount of change of occupation, and to be called upon to take such a survey of the subject in connection with which his researches are made, as is involved in the delivery of a course of lectures, and other details of teaching. Though it is not a thing to be contemplated, that the researcher shall sell his instruction at a price sufficiently high to enable him to live by teaching, yet it is a good thing to make teaching an additional and subsidiary part of his life's work. This end is effected in Germany by making it a duty of the professor (already supported by a stipend) to give some five or six lectures a week during the academical session, for which he is paid by the fees of his hearers. The fees are low, but are sufficient to be an inducement; and, inasmuch as the attendance of the students is not compulsory, the professor is stimulated to produce good and effective lectures at a reasonable charge, so as to attract pupils who would seek instruction from some one else, if the lectures were not good, or the fees too high. Indeed, in Germany this system works so much to the advantage of the students, that the private teachers of the universities at one time obtained the creation of a regulation forbidding the professors to reduce their fees below a certain minimum; since, with so low a fee as some professors were charging, it was impossible for a private teacher to compete. This state of things may be compared with much advantage with the condition of British universities. In these we hear, from one direction, complaints of the high fees charged, and of the ineffective teaching given by the professoriate; and in other universities, where no adequate fees are allowed to the professors as a stimulus to them to offer useful and efficient teaching, we find that the teaching has passed entirely out of their hands into those of college tutors and lecturers. The fact is, that a satisfactory relation between teaching and research is one which will not naturally and spontaneously arrange itself. It can hardly be said to exist in any British university or college, but the method has been thought out and carried into practice in Germany. It consists in giving a competent researcher a stipend, and a laboratory for his research work, and then requiring him to do a small amount of teaching, remunerated by fees proportionate to his ability and the pains which he may take in his teaching. If you pay him a fixed sum as a teacher, or artificially insure the attendance of his class, instead of letting this part of his income vary simply and directly with the attractiveness of his teaching, you will find as the result that (with rare exceptions) he will not give effective and useful teaching. He will naturally tend to do the minimum required of him in a perfunctory way. On the other hand, if you leave him without stipend as a researcher, dependent on the fees of pupils for an income, he will give all his time and energies to teaching: he will cease to do any research, and become, *pro tanto*, an inferior teacher.

A third objection which is sometimes made to the proposition that scientific research must be supported and paid for as such, is the following: it is believed by many persons that a man who occupies his best energies in scientific research can always, if he choose, make an income by writing popular books or newspaper articles in his spare hours; and, accordingly, it is gravely maintained that there is no need to provide stipends, and the means of carrying on their work, for researchers. To do so, according to this view, would be to encourage them in an exclusive reticence, and to remove from them the inducement to address the public on the subject of their researches, by which the public would lose valuable instruction.

This view has been seriously urged, or I should not here notice it. Any one who is acquainted with the sale of scientific books, and the profits which either

author or publisher makes by them, knows that the suggestion which I have quoted is ludicrous. The writing of a good book is not a thing to be done in leisure moments; and such as have been the result of original research have cost their authors often years of labor, apart from the mere writing. Mr. Darwin's books, no doubt, have had a large sale; but that is due to the fact, apart from the exceptional genius of the man who wrote them, that they represent some thirty or more years of hard work, during which he was silent. There is not a sufficiently large public interested in the progress of science to enable a researcher to gain an income by writing books, however great his literary facility. A schoolbook or classbook may now and then add more or less to the income of a scientific investigator; but he who becomes the popular exponent of scientific ideas, except in a very moderate and limited degree, must abandon the work of creating new knowledge. The professional *littérateur* of science is as much removed by his occupation from all opportunity of serious investigation as is the professional teacher who has to consume all his time in teaching. Any other profession — such as the bar, medicine, or the church — is more likely to leave one of its followers time and means for scientific research than is that of either the popular writer or the successful teacher.

We have, then, seen that there is no escape from the necessity of providing stipends and laboratories for the purpose of creating new knowledge, as is done in continental states, if we are agreed that more of this new knowledge is needed, and is among the products which a civilized community is bound to turn out, both for its own benefit and for that of the community of states, which give to and take from one another in such matters.

There are some who would finally attack our contention by denying that new knowledge is a good thing, and by refusing to recognize any obligation, on the part of England, to contribute her share to that common stock of increasing knowledge by which she necessarily profits. Among such persons are those who would prohibit altogether the pursuit of experimental physiology in England, and yet would not and do not hesitate to avail themselves of the services of medical men whose power of rendering those services depends on the fact that they have learned the results obtained by the experiments of physiologists in other countries or in former times. In reference to this strange contempt and even hatred of science, which undoubtedly has an existence among some persons of consideration even at the present day, I shall have a few words to say before concluding this address. I have now to ask you to listen to what seems to me to be the demand which we should make, as members of a British association for the advancement of science, in respect of adequate provision for the creation of new knowledge in the field of biology in England.

Taking England alone, as distinct from Scotland and Ireland, we require, in order to be approximately on a level with Germany, forty new biological institutes, distributed among the five branches of physiology, zoölogy, anatomy, pathology, and botany, — forty, in addition to the fifteen which we may reckon (taking one place with another) as already existing. The average cost of the buildings required would be about £4,000 for each, giving a total initial expenditure of £160,000; the average cost of stipends for the director, assistants, and maintenance, we may calculate at £1,500 annually for each, or £60,000 for the forty, — equal to a capital sum of £2,000,000. These institutes should be distributed in groups of five — eight groups in all — throughout the country. One such group would be placed in London (which is at present almost totally destitute of such arrangements), one in Bristol, one in Birmingham, one in Nottingham, one in Leeds, one in Newcastle, one in Ipswich, one in Cardiff, one in Plymouth, — in fact, one in each of the great towns of the kingdom where there is at present, or where there might be with advantage, a centre of professional education and higher study. The first and the most liberally arranged of these biological institutes — embracing its five branches, each with its special laboratory and staff — should be in London. If we can have nothing else, surely we may demand, with some hope that our request will eventually obtain compliance, the formation in London of a College of scientific research similar to that of Paris (the Collége de France). It is one of the misfortunes and disgraces of London, that, alone amongst the capitals of Europe, with the exception of Constantinople, it is destitute of any institution corresponding to the universities and colleges of research which exist elsewhere.

Either in connection with a properly organized teaching university, or as an independent institution, it seems to me a primary need of the day that the government should establish in London laboratories for scientific research. Two hundred and fifty years ago Sir Thomas Gresham founded an institution for scientific research in the city of London. The property which he left for this purpose is now estimated to be worth three millions sterling. This property was deliberately appropriated to other uses, by the Corporation of the city of London and the Mercers' company, about a hundred years since, with the consent of both Houses of Parliament. By this outrageous act of spoliation these corporations, who were the trustees of Gresham, have incurred the curse which he quaintly inserted in his will in the hope of restraining them from attempts to divert his property from the uses to which he destined it. 'Gresham's curse' runs as follows: "And that I do require and charge the said Corporations and chief governors thereof, with circumspect Diligence and without long Delay, to procure and see to be done and obtained, as they will answer the same before Almighty God; (for if they or any of them should neglect the obtaining of such Licenses or Warrants, which I trust cannot be difficult, nor so chargeable, but that the overplus of my Rents and Profits of the Premisses hereinbefore to them disposed, will soon recompense the same; because to soe good Purpose in the Commonwealth, no Prince nor Council in any Age, will deny or defeat the same. And if conveniently by my Will or other Convenience, I might assure it, I would not leave it

to be done after my death, then the same shall revert to my heirs, whereas I do mean the same to the Commonwealth, and then THE DEFAULT THEREOF SHALL BE TO THE REPROACH AND CONDEMNATION OF THE SAID CORPORATIONS AFORE GOD"). I confess that I find it difficult to see how the present representatives of the corporations who perverted Gresham's trust are to escape from justly deserving the curse pronounced against those corporations, unless they conscientiously take steps to restore Gresham's money to its proper uses. Let us hope that Gresham's curse may be realized in no more deadly form than that of an act of parliament repealing the former one which sanctioned the perversion of Gresham's money. Such a sequel to the report of the commission which has recently inquired into the proceedings of the corporation and companies of the city of London is not unlikely.

Whilst we should, I think, especially press upon public attention the need for an institute of scientific research in London, and indicate the source from which its funds may be fitly derived, we must also urge the foundation of other institutes in the provinces, upon the scale already sketched; because it is only by the existence of numerous posts, and of a series of such posts, — some of greater and some of less value, the latter more numerous than the former, — that any thing like a professional career for scientific workers can be constructed. It is especially necessary to constitute what I have termed 'assistantships,' that is, junior posts, in which younger men assist, and are trained by, more experienced men. Even in the few institutions which do already exist, additional provision of this kind is what is wanted more than any thing else, so that there may be a progressive career open to the young student, and a sufficient field of trained investigators from which to select in filling up the vacancies in more valuable positions.

I am well aware that it will be said that the scheme which I have proposed to you is gigantic and almost alarming in respect of the amount of money which it demands. One hundred and sixty thousand pounds a year for biology alone must seem, not to my hearers, but to those who regard biology as an amusing speculation, — that is to say, who know little or nothing about it, — an extravagant suggestion. Unfortunately, it is also true that such persons are very numerous, — in fact, constitute an overwhelming majority of the community; but they are becoming less numerous every day. The time will come, it seems possible, when there will be more than one member of the government who will understand and appreciate the value of scientific research. There are already a few members of the House of Commons who are fully alive to its significance and importance.

We may have to wait for the expenditure of such a sum as I have named, and possibly it may be derived ultimately from local rather than imperial sources, though I do not see why it should be; yet I think it is a good thing to realize *now* that this is what we ought to expend in order to be on a level with Germany. This apparently extravagant and unheard of appropriation of public money *is actually made every year in Germany.*

I think it is well to put the matter before you in this definite manner; because I have reason to believe that even those whom we might expect to be well informed in regard to such matters are not so, and, as a consequence, there is not that keen sense of the inferiority and inadequacy of English arrangements in these matters which one would gladly see actuating the conduct of English statesmen. For instance: only a few years ago, when speaking at Nottingham, the present prime-minister, who has taken an active part in re-arranging our universities, and has, it is well known, much interest in science and learning, stated that £27,000, the capital sum expended on the Nottingham college of science, was a very important contribution to the support of learning in this country, amounting, as he said he was able to state from the perusal of official documents, to as much as one-third of what was spent in Germany during the past year upon her numerous universities, which were so often held up to England as an example of a well-supported academical system. Now, I do not think that Mr. Gladstone can ever have had the opportunity of considering the actual facts with regard to German universities: for he was in this instance misled by the official return of expenditure on a single university, namely, that of Strasburg; the total annual expenditure on the twenty-one German universities being, in reality, about £800,000, by the side of which a capital sum of £27,000 looks very small indeed. I cannot but believe, that if the facts were known to public men, in reference to the expenditure incurred by foreign states in support of scientific inquiry, they would be willing to do something in this country of a sufficient and statesmanlike character. As it is, the concessions which have been made in this direction appear to me to be in some instances not based upon a really comprehensive knowledge of the situation. Thus, the tentative grant of £4,000 a year from the treasury to the Royal society of London appears to me not to be a well-devised experiment in the promotion of scientific research by means of grants of money; because it is on too small a scale to produce any definite effect, and because the money cannot be relied upon from year to year as a permanent source of support to any serious undertaking.

The Royal society most laboriously and conscientiously does its best to use this money to the satisfaction of the country, but the task thus assigned to it is one of almost insurmountable difficulty. In fact, no such miniature experiments are needed. The experiment has been made on a large scale in Germany, and satisfactory results have been obtained. The reasonable course to pursue is to benefit by the experience, as to details and methods of administration, obtained in the course of the last sixty years in Germany, and to apply that experience to our own case.

It is quite clear that 'the voluntary principle' can do little towards the adequate endowment of scientific research. Ancient endowments belonging to the country must be applied thereto, or else local or imperial taxes must be the source of the necessary support.

Seeing that the results of research are distinctly of imperial and not of local value, it would seem appropriate that a portion of the imperial revenue should be devoted to their achievement. In fact, as I have before mentioned, the principle of such an application of public money has long been admitted, and is in operation.

Whilst voluntary donations on the part of private persons can do little to constitute a fund which shall provide the requisite endowment for the scheme of biological institutes which I have sketched (not to mention those required for other branches of science), yet those who are interested in the progress of scientific investigation may, by individual effort, do something, however little, towards placing research in a more advantageous position in this country. Supposing it were possible, as I am sanguine enough to believe that it is, to collect in the course of a year or two, from private sources, a sum of £20,000 for the maintenance of a biological laboratory and staff: it would be necessary, in expending so limited a sum, to aim at the provision of something which would be likely to produce the largest and most obvious results in return for the outlay, and to benefit the largest number of scientific observers in this department.

I believe that it is the general opinion among biologists, that there could be no more generally useful institution thus set in operation than a biological laboratory upon the seacoast, which, besides its own permanent staff of officers, would throw open its resources to such naturalists as might from time to time be able to devote themselves to researches within its precincts. There is no such laboratory on the whole of the long line of British coast. At Naples there is Dr. Dohrn's celebrated and invaluable laboratory, which is frequented by naturalists from all parts of the world; at Trieste, the Austrian government supports such a laboratory; at Concarneau, Roscoff, and Villefranche, the French government has such institutions; at Beaufort, in North Carolina, the Johns Hopkins university has its marine laboratory; and at Newport Professor Alexander Agassiz has arranged a very perfect institution also for the study of marine life. In spite of the great interest which English naturalists have always taken in the exploration of the sea and marine organisms; in spite of the fact that the success, and even the existence, of our fisheries industries, to a large extent depend upon our gaining the knowledge which a well-organized laboratory of marine biology would help us to gain, — there is actually no such institution in existence.

This is not the occasion on which to explain precisely how, and to what extent, a laboratory of marine zoölogy might be of national importance. I hope to see that matter brought before the section during the course of our meeting. But I may point out now, that though it appears to me that the great need for biological institutes, to which I have drawn your attention, can *not* be met by private munificence, and must, in the end, be arranged for by the continued action of the government in carrying out a policy to which it has for many years been committed, and which has been approved by conservatives and liberals alike, yet such a special institution as a laboratory of marine biology, serving as a temporary workshop to any and all of our numerous students of the important problems connected with the life of marine plants and animals, might very well be undertaken from private funds. Should it be possible, on the occasion of this meeting of the British association in Southport, to obtain some promise of assistance towards the realization of this project, I think we shall be able to congratulate ourselves on having done something, though small, perhaps, in amount, towards making better provision for biological research, and therefore something towards the advancement of science.

In conclusion, let me say, that, in advocating to-day the claim of biological science to a far greater measure of support than it receives at present from the public funds, I have endeavored to press that claim chiefly on the ground of the obvious utility to the community of that kind of knowledge which is called biology. I have endeavored to meet the opposition of those who object to the interference of the state, wherever it may be possible to attain the end in view without such interference, but who profess themselves willing to see public money expended in promoting objects which are of real importance to the country, and which cannot be trusted to the voluntary enterprise arising from the operation of the laws of self-preservation, and the struggle for wealth. There are, however, it seems to me, further reasons for desiring a thorough and practical recognition by the state of the value of scientific research. There are not wanting persons of some cultivation, who have perceived and fully realized the value of that knowledge which is called science, and of its methods, and yet are anxious to restrain rather than to aid the growth of that knowledge. They find in science something inimical to their own interests, and accordingly either condemn it as dangerous and untrustworthy, or encourage themselves to treat it with contempt by asserting, that, 'after all, science counts for very little,' — a statement which is unhappily true in one sense, though totally untrue when it is intended to signify that the progress of science is not a matter which profoundly influences every factor in the well-being of the community. Amongst such people there is a positive hatred of science, which finds expression in their exclusion of it, even at this day, from the ordinary curriculum of public-school education, and in the baseless, though oft-repeated calumny, that science is hostile to art, and is responsible for all that is harsh, ugly, and repulsive in modern life. To such opponents of the advancement of science it is of little use to offer explanations and arguments. But we may, when we reflect on their instinctive hostility, and the misrepresentations of science and the scientific spirit which it leads them to disseminate, console ourselves by bringing to mind what science really is, and what truly is the nature of that calling in which a man who makes new knowledge is engaged.

They mock at the botanist as a pedant, and the zoölogist as a monomaniac; they execrate the physi-

ologist as a monster of cruelty, and brand the geologist as a blasphemer; chemistry is held responsible for the abomination of aniline dyes and the pollution of rivers, and physics for the dirt and misery of great factory towns. By these unbelievers, science is declared responsible for individual eccentricities of character, as well as for the sins of the commercial utilizers of new knowledge. The pursuit of science is said to produce a dearth of imagination, incapability of enjoying the beauty either of nature or of art, scorn of literary culture, arrogance, irreverence, vanity, and the ambition of personal glorification.

Such are the charges, from time to time, made by those who dislike science; and for such reasons they would withhold, and persuade others to withhold, the fair measure of support for scientific research which this country owes to the community of civilized states. Not in reply to these misrepresentations, but by way of contrast, I would here state what science seems to be to those who are on the other side, and how, therefore, it seems to them wrong to delay in doing all that the wealth and power of the state can do to promote its progress.

Science is not a name applicable to any one branch of knowledge, but includes all knowledge which is of a certain order or scale of completeness. All knowledge which is deep enough to touch the causes of things is science: all inquiry into the causes of things is scientific inquiry. It is not only co-extensive with the area of human knowledge, but no branch of it can advance far, without reacting upon other branches: no department of science can be neglected, without sooner or later causing a check to other departments. No man can truly say this branch of science is useful, and shall be cultivated, whilst this is worthless, and shall be let alone: for all are necessary; and one grows by the aid of another, and in turn furnishes methods and results assisting in the progress of that from which it lately borrowed.

We desire the increase and the support and the acceptance of science, not only because it has a certain material value, and enables men to battle with the forces of nature, and to turn them to account so as to increase both the intensity and the extension of healthy human life: that is a good reason, and for some persons, it may be, the only reason. But there is something to be said beyond this.

The pursuit of scientific discovery, the making of new knowledge, gratifies an appetite, which, from whatever cause it may arise, is deeply seated in man's nature, and, indeed, is the most distinctive of his properties. Man owes this intense desire to know the nature of things, smothered though it often be by other cravings which he shares with the brutes, to an inherited race-perception, stronger than the reasoning faculty of the individual. When once aroused, and in a measure gratified, this desire becomes a guiding passion. The instinctive tendency to search out the causes of things, gradually strengthening as generation after generation of men have stumbled and struggled in ignorance, has at last become an active and widely extending force: it has given rise to a new faith.

To obey this instinct — that is, to aid in the production of new knowledge — is the keenest and the purest pleasure of which man is capable, greater than that derived from the exercise of his animal faculties in proportion as man's mind is something greater and further developed than the mind of brutes. It is in itself an unmixed good, the one thing which commends itself as still 'worth while' when all other employments and delights prove themselves stale and unprofitable.

Arrogant and foolish as those men have appeared, who, in times of persecution, and in the midst of a contemptuous society, have, with an ardor proportioned to the prevailing neglect, pursued some special line of scientific inquiry, it is nevertheless true that in itself, apart from special social conditions, science must develop, in a community which honors and desires it before all things, qualities and characteristics which are the highest, the most human of human attributes. These are, firstly, the fearless love and unflinching acceptance of truth; hopeful patience; that true humility which is content not to know what cannot be known, yet labors and waits; love of Nature, who is not less, but more worshipped by those who know her best; love of the human brotherhood, for whom and with whom the growth of science is desired and effected.

No one can trace the limits of science, nor the possibilities of happiness, both of mind and body, which it may bring in the future to mankind. Boundless though the prospect is, yet the minutest contribution to the onward growth has its absolute and unassailable value, — once made, it can never be lost: its effect is forever in the history of man.

Arts perish, and the noblest works which artists give to the world. Art, though the source of great and noble delights, cannot create nor perpetuate: it embodies only that which already exists in human experience, whilst the results of its highest flights are doomed to decay and sterility. A vain regret, a constant effort to emulate or to imitate the past, is the fitting and laudable characteristic of art at the present day. There is, indeed, no truth in the popular partition of human affairs between science and art as between two antagonistic or even comparable interests; but the contrast which they present in points such as those just mentioned is forcible. Science is essentially creative: new knowledge — the experience and understanding of things which were *previously non-existent for man's intelligence* — is its constant achievement. And these creations never perish: the new is built on, and incorporates, the old; there is no turning back to recover what has lapsed through age; the oldest discovery is even fresher than the new, yielding in ever-increasing number new results, in which it is itself reproduced and perpetuated, as the parent in the child.

This, then, is the faith which has taken shape in proportion as the innate desire of man for more knowledge has asserted itself: namely, that there is no greater good than the increase of science; that through it all other good will follow. Good as science is in itself, the desire and search for it is even

better, raising men above vile things and worthless competitions, to a fuller life and keener enjoyments. Through it we believe that man will be saved from misery and degradation, not merely acquiring new material powers, but learning to use and to guide his life with understanding. Through science he will be freed from the fetters of superstition. Through faith in science he will acquire a new and enduring delight in the exercise of his capacities: he will gain a zest and interest in life such as the present phase of culture fails to supply.

In opposition to the view that the pursuit of science can obtain a strong hold upon human life, it may be argued, that on no reasonable ground can it appear a necessary or advantageous thing to the individual man to concern himself with the growth and progress of that which is merely likely to benefit the distant posterity of the human race. Our reply is, let those who contend for the reasonableness of human motives develop, if they can, any theory of human conduct in which reasonable self-interest shall be man's guide. We do not contend for any such theory. By reasoning we may explain and trace the development of human nature, but we cannot change it by any such process. It is demonstrably unreasonable for the individual man, guided by self-interest, to share the dangers and privations of his brother-man; and yet, in common with many lower animals, he has an inherited quality which makes it a pleasure to him to do so. It is unreasonable for the mother to protect her offspring, and yet it is the natural and inherited quality of mothers to derive pleasure from doing so. It is unreasonable for the half-starved poor to aid their wholly-starving brethren; and yet such compassion is natural and pleasurable to those who show it, and is the constant rule of life. Unreasonable though these things are, from the point of view of individual self-interest, yet they are done because to do them is pleasurable, to leave them undone a pain. The race has, as it were, in these respects, befooled the individual, and, in the course of evolution, has planted in him, in its own interests, an irrational capacity for taking pleasure in doing that which no reasoning in regard to self-interest could justify. As with these lower and more widely distributed instincts, shared by man with some lower social animals, so is it with this higher and more peculiar instinct, — the tendency to pursue new knowledge. Whether reasonable or not, it has, by the laws of heredity and selection, become part of us, and exists. Its operation is beneficial to the race. Its gratification is a source of keen pleasure to the individual, — an end in itself. We may safely count upon it as a factor in human nature. It is in our power to cultivate and develop it, or, on the other hand, to starve and distort it for a while, though to do so is to waste time in opposing the irresistible.

As day by day the old-fashioned stimulus to the higher life loses the dread control which it once exercised over the thoughts of men, the pursuit of wealth, and the indulgence in fruitless gratifications of sense, become to an increasing number the chief concerns of their mental life. Such occupations fail to satisfy the deep desires of humanity: they become wearisome and meaningless, so that we hear men questioning whether life be worth living. When the dreams and aspirations of the youthful world have lost their old significance, and their strong power to raise men's lives, it will be well for that community which has organized in time a following of and a reverence for an ideal good, which may serve to lift the national mind above the level of sensuality, and to insure a belief in the hopefulness and worth of life. The faith in science can fill this place. The progress of science is an ideal good, sufficient to exert this great influence.

It is for this reason, more than any other (as it seems to those who hold this faith), that the progress and diffusion of scientific research, its encouragement and reverential nurture, should be a chief business of the community, whether collectively or individually, at the present day.

NOTES AND NEWS.

PURSUANT to the invitation already noted in SCIENCE, a number of gentlemen met in the library of the American museum of natural history in New-York City, on the 26th to 28th of September, and founded the American ornithologists' union. The membership consists of active, foreign, corresponding, and associate members. The active membership is limited to fifty residents of the United States and Canada; the foreign, to twenty-five non-residents of the United States and Canada; the corresponding, to one hundred residents of any country; the associate being composed of any number of residents of the United States and Canada. The officers of the union for the current year are, Mr. J. A. Allen, president; Dr. Elliott Coues and Mr. Robert Ridgway, vice-presidents; Dr. C. Hart Merriam, secretary and treasurer; Messrs. S. F. Baird, George N. Lawrence, H. W. Henshaw, and Montagu Chamberlain, councillors, — these nine officers constituting the council of the union. Dr. Coues presided over the convention, and continued in the chair in the absence of the president. Mr. Allen and Professor Baird, who were unable to be present, were added to the list of founders. After the discussion and adoption of a constitution, submitted by the committee of organization, and the election of officers, a large number of members were elected, raising the active and foreign membership nearly to the limit. The work of the union for the present year was laid out by the formation of committees, appointed by the chair, on the subjects of classification and nomenclature, of the distribution and migration of birds, of avian anatomy, of oölogy, and on the question of the eligibility or ineligibility of the European sparrow in America. The first-named committee, besides revising the current lists of North-American birds, is expected to consider the subject of zoölogical nomenclature at large; and its labors may result in the formation of a code of nomenclature applicable to other departments of zoölogy, as well as to ornithology. It consists of Messrs. Ridgway, Allen, Brewster, Henshaw, and Coues.

— Mr. Charles F. Parker, the curator in charge of the Academy of natural sciences of Philadelphia, died Sept. 7, after an illness of several months. Mr. Parker became a member of the academy in 1865, and was elected a curator in 1873. Shortly afterward he was appointed by the council curator in charge, — a position which he filled with singular efficiency until last March, when he was compelled to avail himself of leave of absence, granted by the council in the hope that he would soon be able to return. Mr. Parker had paid special attention to the botany of New Jersey; and, both in the completeness of his herbarium and the accuracy of his knowledge of it, he had few, if any, equals. Even before his connection with the academy, he was well known to the leading botanists of America, and his collection was frequently referred to by specialists for illustrative material. The many students who have visited the academy during his term of office will remember the alacrity with which he rendered them assistance in their investigations. Although he may be succeeded by one having a more general knowledge of natural history in its several departments, or a more profound knowledge of a specialty, the academy will probably not be able to secure the services of any one person able and willing to perform the same work so economically and efficiently.

— We copy from the daily press the following telegram from Lieut. Ray, commanding the Point-Barrow expedition, concerning whose safety there were reasonable grounds for anxiety: —

"San Francisco, Oct. 7, 1883. — I report my safe arrival here to-day with party. Also brought down Lieut. Schwatka and party from St. Michaels. All work accomplished as ordered by chief signal-officer. Pendulum observation not made. Leo reached Ooglaamie Aug. 22; was driven away by ice the same night; returned on the 24th; again driven away and damaged on the 25th; returned on the 27th, when party and stores were embarked; sailed on the 29th, vessel leaking badly; put into Unalaska, where she was beached and repaired."

— A large and exceptionally fine collection of fossil plants from the Fort-Union group (Laramie) is now on its way to Washington, collected in the valley of the Yellowstone River, within thirty miles of Glendive, Montana, by Mr. Lester F. Ward, assisted by Mr. Richard Foster. Mention has already been made (Science, i. 559) of a small but interesting collection from this locality, which was partially elaborated last spring. The same stations were revisited and thoroughly worked. The expedition was very successful, and the collection is one of the largest and best ever made in the country. Fifty-seven boxes of fossils, aggregating nearly four tons in gross weight, were obtained. The material was carefully assorted, and scarcely any but cabinet specimens were taken. In the very large number of genera and species represented, there can scarcely fail to be some new to science. The localities examined embrace several distinct horizons within the group, each possessing a special facies. Nearly all the old forms described by Dr. Newberry appear in abundance, — Populus, Platanus, Viburnum, Rhamnites, Tilia, etc., — but varied by additional species; while such new genera as Trapa, Rhamnus, Ilex, Eleodendron, Asarum, Ficus, etc., are present, often in great profusion, and beautifully preserved. Special pains were taken to secure as large and complete a representation as possible of those forms whose affinities are less obvious or wholly unknown. Mr. Ward intends to commence work on this collection as soon as it arrives.

— The 13th of August, 1883, was the hundredth anniversary of the successful attempt of the brothers Montgolfier to cause their hot-air balloon to rise. On that day a monument commemorative of the

event was unveiled at Annonay, where the Montgolfiers lived and worked. Joseph, the older, is represented as holding the balloon, while his younger brother, Étienne, fills it with heated air by means of a lighted torch. For the three days the streets of Annonay were filled with the crowds gathered to honor the memory of the great inventors. In the addresses stress was laid upon the aids which the use of the balloon may be to the sciences, especially meteorology, and in military operations. Joseph Montgolfier was born at Vidalon-les-Annonay, Aug. 26, 1740, and Étienne at the same place, the 7th of January, 1745. The younger brother died Aug. 2, 1799, at Serrière; and Joseph, after a stroke of paralysis in 1809, died at Balaruc-les-Bains on the 26th of June, 1810.

— A notable event of the present season's field-

work has been the descent of the Missouri River in a 'Mackinaw' (a sort of flat boat) from Fort Benton to Bismarck by a party of geologists, consisting of Dr. C. A. White, Mr. J. B. Marcou, and Mr. Lester F. Ward, with one assistant, for the express purpose of geological and paleontological study.

The distance, according to steamboat schedule, is 1,059 miles; and thirty days (Aug. 22 to Sept. 20) were consumed in the journey. A large part of the territory passed through is occupied by Indian reservations; and there is no white population between Benton and Poplar Creek Agency, the first post-office, — a distance of 567 miles. The river is very low at this season of the year; and the current was correspondingly sluggish, though still quite rapid enough in some places. Progress was farther impeded by shoals, bars, and head winds; and considerable time was, of course, occupied in climbing and examining the adjacent bluffs and mountains.

The geology of this region, as all know, is very interesting; and the trip is believed to have thrown much light upon some of its leading problems. The results of the expedition will, of course, be officially made known in due time by the several parties participating, who have brought with them ample data, both in the form of notes and specimens.

— Mr. G. Brown Goode arrived in Washington on the 2d inst. from London.

— Representatives of nearly all the branches of the western surveys have returned to Washington. Dr. C. A. White reports having explored a great number of miles of the upper Missouri in a row-boat, being engaged in extending and confirming his previous observations of the formations.

— The winter session of the Philosophical society of Washington opens on the 13th inst. A considerable number of communications on widely different topics are in readiness. Biological papers are not numerous. The Biological society will probably begin its session on the 19th inst. It is possible that negotiations for the formation of a Washington academy of sciences will be opened for a second time this winter, but with what success it is impossible at present to say. It seems to be generally considered that an academy would be desirable, but there is little agreement relative to the proper basis of union between the existing societies.

— Prof. K. A. Zittel of Munich is visiting this country, and will probably be in Washington early in this month.

— At the first autumn meeting of the Boston society of natural history, Oct. 3, Mr. F. W. Putnam gave an account of the great serpent-mound in Adams county, O., and of some other ancient works in Wisconsin and Ohio examined during the past summer.

— The Appalachian mountain-club commenced its Boston meetings on the 10th, when papers were read on the Route Salvan, by Selah Howell; on a trip over Osceola, the Twin Mountain range, and Garfield, by W. L. Hooper; and on an exploration of the Traveller Mountain, and the head waters of Mattagamon River, by G. H. Witherle.

RECENT BOOKS AND PAMPHLETS.

Aymard, J. La poudre à canon; le télégraphe; les montagnes et les volcans; les tremblements de terre, les pétrifications. Paris, *Lefort*, 1883. 107 p. 12°.

Barrois, T. Catalogue des crustacés podophtalmaires et des échinodermes recueillis à Concarneau durant les mois d'août-septembre, 1880. Lille, *impr. Danel*, 1883. 68 p., pl., map. 8°.

Berquin. Les merveilles du firmament, ou le système de la nature dévoilé à la jeunesse. Limoges, *Ardant*, 1883. 119 p. 8°.

Bonnet, E. Petite flora parisienne, contenant la description des familles, genres, espèces et variétés de toutes les plantes spontanées ou cultivées en grand dans la région parisienne, avec des clefs dichotomique conduisant rapidement aux noms des plantes; augmentée d'un vocabulaire. Paris, *Savy*, 1883. 12+528 p. 18°.

Brass, A. Biologische studien. Theil I.: Die organisation der thierischen zelle. 8°.

Broca, P. Memoires d'anthropologie. Paris, *Reinwald*, 1883. 800 p. 8°.

Chatenet, E. du. Pompéi et Herculanum, découverte et description de ces deux villes romaines. Limoges, *Ardant*, 1883. 120 p. 12°.

Cole, E. M. Geological rambles in Yorkshire: a popular handbook of magnesian limestone, new red sandstone, etc. London, *Simpkin*, 1883. 112 p. 8°.

Costatin, J. Étude comparée des tiges aériennes et souterraines des dicotylédones. Paris, *Masson*, 1883. 177 p., 8 pl. 8°.

D'Anvers, U. Flowerless plants. London, *Philip*, 1883. (Sc. ladders.) 84 p. 12°.

De Long, Emma. The voyage of the Jeannette; the ship and ice journals of George W. De Long. Edited by his wife. 2 vols. Boston, *Houghton, Mifflin, & Co.*, 1883. 22+911 p., 2 portr., 5 maps, 14 pl., illustr. 8°.

Dubois, A. La science populaire. Au bord d'une mare, entretiens sur l'histoire naturelle. Limoges, *Ardant*, 1883. 304 p. 4°.

—— Les animaux dans les bois. Limoges, *Ardant*, 1883. 192 p. 8°.

—— Les oiseaux et les insectes. Limoges, *Ardant*, 1883. 191 p. 8°.

—— Les végétaux dans les bois. Limoges, *Ardant*, 1883. 192 p. 8°.

Duclau, S. La science populaire; la chaleur et ses effets. Limoges, *Ardant*, 1883. 120 p. 12°.

Exposition internationale d'électricité, Paris, 1881. Jury reports. 2 vols. Paris, *Masson*, 1883. 484; 414 p. 8°.

Frenzel, J. Ueber bau und thätigkeit des verdauungskanals der larve der Tenebrio moliter mit berücksichtung anderer arthropoden. Inaug. diss. Göttingen, *Vandenhoeck & Ruprecht*, 1882. 50 p. 8°.

Govin, M., and **Moireau**, M. Notions de cosmographie. Paris, *Bertaux*, 1883. 36 p. 18°.

Gresley, W. S. A glossary of terms used in coal-mining. London, *Spon*, 1883. 306 p., illustr. 8°.

Heaford, A. S. Strains on braced iron arches and arched iron bridges. London, *Spon*, 1883. illustr. 8°.

Illinois — Geological survey. Vol. 7. Geology and paleontology, by A. H. Worthen. Paleontology, by A. H. Worthen, O. St. John, and S. A. Miller; with an addenda (*sic*) by C. Wachsmuth and W. H. Barris. (Springfield), *State*, 1883. (4)+373 p., 31 pl. l. 8°.

Kutscher, E. Die verwendung der gertsäure im stoffwechsel der pflanze. Inaug. diss. Göttingen, *Vandenhoeck & Ruprecht*, 1883. 36 p., 2 pl. 8°.

Lackemann, W. Euler's interpolirte producte. Inaug. diss. Göttingen, *Vandenhoeck & Ruprecht*, 1882. 43 p. 8°.

Lange, E. Goethe's farbenlehre vom standpuncte der wissenschaftstheorie und aesthetik. Inaug. diss. Göttingen, *Vandenhoeck & Ruprecht*, 1882. 38 p. 8°.

Leydig, F. Untersuchungen zur anatomie und histologie der thiere. 8°.

Mouillefert, P. Vignes phylloxerées; faits établissant l'efficacité et la haute valeur du sulfocarbonate de potassium pour combattre le phylloxera, etc. Paris, *Narbonne*, 1883. 58 p. 4°.

Munro, J. Electricity and its uses. London, *Tract society*, 1883. illustr. 8°.

Pichler, M. von. L'indicateur du travail et du fonctionnement des machines à piston, à vapeur, à eau, à gaz, etc., et son diagramme. Traduit par R. Seguela. Paris, *Baudry*, 1883. 5+98 p., 46 fig. 8°.

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